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ATTENTION-AUTOMATIZATION: AN INVESTIGATION OF THE TRANSITIONAL NATURE OF MIND[1]

By Adelbert Ford, University of Michigan

I. Experimental Aims

One occasionally hears the caustic comment that research is conducted in order to discover proof for preconceived theories rather than to formulate principles after the empirical evidence has been attained by strict laboratory methods. The researcher must be careful lest he be found among the dogmatists, who allow their biases to control their experiments. But it is evident that experimentation without a planned attack, without at least some warning of possible hypotheses to be developed, leads nowhere. The antecedent formulation of psychological theorems must be held as a scientific sin only when the worker is not frankly open to both favorable and unfavorable evidence.

The working hypotheses for our experiment were developed from a critical review of certain historical experiments on the nature of that supposed psychological state or process to which most texts refer as *attention*. More precisely stated, we are indebted to the work of J. J. B. Morgan[2] on distraction. Although terms, no matter how evasive, tend to commit one to conventional interpretations, we wonder to what extent Morgan's evasion of the word *attention*, or of any other term which signifies a theory, may have prevented an arrangement of experimental data, well done from the standpoint of laboratory technique, from showing certain interesting principles of mental integration.

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[1] From the Department of Psychology, University of Michigan.

[2] J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 5, 1916, (no. 35), 1-84.

Although the term *attention* has been attached, in most of the historical researches, to a description of consciousness, there seems to be no great difficulty in showing attributes of behavior which are comparable to the structuralistic descriptions. Certainly it is not impossible to give Titchener's definition of the focal point of consciousness a strictly behavioristic formulation. One need not observe an animal for a very long period before seeing fairly definite indications of 'focal' and 'background' coördinations. Neither does it seem unreasonable to hold that there are definite indications of a sequence of acts, determined by previous environmental conditions, which evolve from each other in a selective order. Nevertheless, the dominance of a coördination and the corresponding suppression of acts which are not serving the same end as the coördination seem to be a purely relative matter, for there are types of coördination which interfere with each other to a minor extent only, and others which seem to be thoroughly antagonistic. An examination of the life-history of these coördinations appears to reveal something in the interference which is dependent upon a lack of repetitive use.

For example, one finds when learning to drive an automobile that it is practically impossible to perform the several acts which are required in a sudden emergency without a great amount of interference. Although each of the acts is consummated easily enough when carried out independently, simultaneity is impossible. When one has learned to drive the car well, however, one can actually perform the various acts of stopping with perfect synchronism while conversing. The mere fact of use seems to bring about a complete cessation of mutual interference and it may also give indications that the various components of the coördination are more efficient when carried out in the presence of each other.

The interference of one stimulus against another is so common an occurrence in everyday life as to suggest one of the most urgent problems concerning the 'process' nature of mind in the sense of a transition from new syntheses to habituated coördinations—from attention to automatization. We hear much about the structure of consciousness or the classification of behavior and very little about the transitional nature of mind; although many experiments have contained the principles in a mass of data better arranged to conceal than to demonstrate.

The history of work on distraction therefore prompts the following theorems.

(1) We may not frame any hypothesis in such a manner as to invalidate the known facts of nervous functions. The general subject of the acquisition of neural integrations may, when descriptive of mental actions or operations, be referred to under the

general phrase *psycho-dynamic gradients*, whether viewed from the standpoint of behavior, of neural function or of conscious phenomena.

(2) The repeated use of similar pattern-stimuli must reduce behavior to such a condition that specific interference is no longer present.

(3) Behavior selected under one set of conditions is a mental act different from the same behavior selected under other conditions. We argue for the classification of actions in terms of central determinants, not in terms of ends served.

(4) Automatization is essential in all acts of integration. It begins immediately and proceeds with greater and greater force until the coördination reaches a complete segregation from other systems of behavior. The speed of this tendency may be slow or rapid depending on the complexity of the act selected. *Attention* and *automatization* become the extremes of a transition accomplished through sheer repetition of the same stimulus-pattern.

(5) Resistance and acceptance reactions obey the same law. Avoidance of certain pattern-stimuli may become set off into automatization as well as the acceptance or selection of positive modes of behavior.

(6) There is no fundamental objection to considering attention as a process of association provided we differentiate between initial and subsequent stages. We may indicate, however, that the fact of dominance (clearness) and the facts of the psycho-dynamic sequence are two separate phases for consideration and should not both be labelled *attention*.

(7) A graphic representation called an 'attention' curve, made on the assumption that efficiency of behavior is a measure of attention, cannot remain level and constitute continuous attention. For example, training a subject in the use of problems to attain a level curve of efficiency is but to substitute automatization for attention. It defeats the purpose of the experiment. A slope in the curve of efficiency is in itself evidence that selection is operating and attention is present.

(8) The selection of a given type of behavior is probably never the result of any one condition but rather the result of composite forces of the entire environmental situation, and no measurement of attention is adequate unless all changes in the

environment are taken into consideration. The importance of this principle has grown more pronounced with the appearance of the *Gestalt* movement.

II. HISTORICAL ANTECEDENTS

The nervous basis of reciprocal inhibition in the spinal cord was given its final verification by Sherrington.[3] A certain degree of application of the laws of spinal action to principles of cerebral action seems warranted on the basis of the facts of learning and memory. But we must not fail to note that every part of the spinal cord is connected with every other part. Sherrington writes, "Yet we must not lose sight of the physiological solidarity of the action of the group of elements that compose a 'reflex center' in its reflex activity. 'Immediate spinal induction' and the spatial spread of the refractory phase in the scratch-reflexes evoked from separate points show that, intra-spinally, the various component arcs of the type-reflex are connected to something like a unitary mechanism." The fact that events in one part of the nervous system modify the functioning of other parts ought to make the psychologist cautious in accepting any theory of association built on the concept of a single nervous tract. We cannot escape a mass of cross-relationships of one phase of behavior with another, the shifting of which should suggest many a fine problem in psycho-dynamic gradients.

The fact that researches upon attention have a tendency to become complicated with the process of automatization has been known since the earliest experiments by modern laboratory methods. It seems certain that Wundt was aware of this factor as a serious problem prior to 1880. Cattell[4] reported in 1886 that distraction stimuli were more effective on acts which were attended to for the first time. In spite of the fact that Cattell had published the results of his experiments involving the dangers of automatization, it does not seem to have occurred to anyone for a long time that the effect of distraction might not be a constant, but rather a persistent variable which is correlated with the newness of the integration. Therefore most investigators did not see that a temporal function was an important measure of the effect of distraction on attention. Almost all of the early experiments have assumed that, as long as the attended work was kept as variable as possible, all parts of a temporal sequence were equally well attended to. Often these early experiments have missed the possibility that a persistent common form running through the mental work could breed automatization just as the experienced bookkeeper learns to add new columns of old and familiar elements of organization; or just as the accomplished musician can read automatically a complicated new score because that score is cast in a form which makes use of old and familiar stimulus-patterns. It would seem that the failure to control the temporal aspect has been due to the traditional definition of attention as an attribute of dominance, without an adequate interest in the determinants which lead to that dominance.

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[3] C. S. Sherrington, *The Integrative Action of the Nervous System*, 1923, Lect. 4.

[4] J. McK. Cattell, Psychometrische Untersuchungen, *Phil. Stud.*, 3, 1886, 329; The time taken up by mental operations, *Mind*, 11, 1886, 237-242.

In 1892 Swift[5] belittled the dangers of automatization, although acquainted with the work of Wundt's laboratory and Cattell's published work. Swift noted, however, that the effect of distraction diminished if the interference became "monotonous," failing to note anything in common between monotony and automatization. Apparently he seems to have missed the possibility that tendencies of resistance and acceptance are equally capable of automatization. He argued one out of existence but accepted the other. Swift concluded that the degree of attention was inverse to the amount of distraction from auditory and visual stimuli.

Münsterberg[6] tried the effect of introducing voluntary arithmetical work as a distraction to the process of making judgments on auditory intensities and visual distances. He had no check on the extent to which subjects resorted to attentional fluctuation between arithmetical problems and sensory discrimination, and no attempt was made to measure the extent to which the arithmetical operations had been reduced to automatized processes. Hamlin's[7] check on Münsterberg's experiment supported the former of the above criticisms. Her excellent review of research upon attention to that date gives a general indication of the previous failure to see attention as the termination of a gradient process, of selective conditions which operate differently with practice.

It was near the beginning of the present century that Titchener[8] laid the foundation for a definition of attention derived from his limitation of the field of psychology to the study of conscious processes. There is definite evidence in the writings of Titchener that he was interested in the functional descriptions of mind; but the prevailing problems in psychology at the beginning of the present century favored a static definition. Many of the experiments began on the problem of the relation of attention to sensory intensity and the possibility of the control of dominance by preceding mental conditions was often neglected. The procedure used in Münsterberg's experiment (previously cited) is an example of the point at issue. The prevalent acceptance of the terms *foreground* and *background* of attention without a more critical valuation of the evidence for these terms led to the repetition of Münsterberg's experiment by Hamlin. A careful examination of this experiment showed that the problem did not logically concern the relation of background and foreground clearness at all, but was rather a problem of the selective factors for dominance. Hamlin says, "We notice first the evidence upon the nature and effects of the 'distraction.' The subjects usually found that it acted as a spur rather than a check on attention. Their attention was wholly absorbed in the addition for a part of the interval, but as a rule the distractions were not continuous, and there came free instants in which the stimulus flashed into consciousness with great clearness and distinctness." How many times have the reports on background clearness been due to these "free instants," *i.e.* momentary fluctuations!

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[5] E. J. Swift, Disturbance of the attention during simple mental processes, this JOURNAL, 5, 1892, 1-19.

[6] Hugo Münsterberg, The intensifying effect of attention, this JOURNAL, 1, 1894, 39-44.

[7] A. J. Hamlin, Attention and distraction, this JOURNAL, 8, 1896, 3-66.

[8] E. B. Titchener, Postulates of a structural psychology, *Philos. Rev.*, 7, 1898, 449-465.

Certainly there is little evidence here for a description of background in the usual sense. We must note that the incentives to attend, as a condition of the selective process, are altered the moment the distraction is used. If our criterion holds true that behavior under one set of selective conditions is a different mental act from the same behavior under other selective conditions, then it seems that we have an adequate explanation of Münsterberg's and Hamlin's results, not in terms of the effect of attention on sensory intensity, but rather in terms of the differences in effectiveness of the conditions of attention.

Most of these experimenters averaged together all reactions, whether they came in the beginning, the middle, or at the end of the experiment, failing to take into account the possibility of a change in the nature of the mental process as the individual did the same kind of work over and over again. Have we here the results of automatized acts averaged in with the results of volitional or attended acts without differentiation? Some weak attempts were made to control automatization. The initial 'practice curve' has long been recognized. But even in this control there has been the persistent uncritical assumption that after a subject has passed through the period of practice he is still exhibiting acts of attention. It seems astonishing that so many experimenters have given no indications of the possibility that the practice curve itself may be the only part of the behavior that is really to be called attention, a persistent gradient always found in the initial stages of newly integrated behavior.

Some of the early experiments are interesting examples of the above lack of control. In 1896 Birch,[9] working in the Cornell laboratory, used odors as a method of distracting attention given to the discrimination of pairs of sounds. The experiment was partially lacking in the control of the temporal factor, but there are one or two outstanding results which appear to be important in the development of our theory of attention. Birch deplored the fact that she had not given a great amount of time in the training of her subjects for the mental work which was to constitute a measure of attention. She found that the curve of attention constantly seemed to fall during the intervals of no distraction. As the subjects were given more and more periods of work there seemed to be a tendency for the number of correct judgments to increase under the effects of distraction. She was afraid that this was due to lack of previous training; but there is a possibility that this would always occur under continued distraction and is but an indication of automatized avoidance of the disturbing stimulus.

Darlington and Talbot,[10] in the same laboratory, attempted to show the effect of short musical phrases as a distraction. Their general conclusion was that discrimination was better during distraction than without distraction.

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[9] L. G. Birch, A study of certain methods of distracting the attention, this JOURNAL, 9, 1897, 45-55.

[10] L. Darlington and E. B. Talbot, A study of certain methods of distracting the attention, this JOURNAL, 9, 1898, 332-345.

But again they gave no curves to show how rapidly this capacity to work better under distraction developed, whether it was abrupt and constant, or gradual and accumulative. All we can conclude from this experiment is that some conditions for selection are better than others. Fear that one is going to be bothered is apparently an indication of a special selective condition and may be accompanied by a pronounced difference in efficiency of behavior. The neglect of the temporal attribute seems to be a persistent tendency among investigators holding a theory of attention as a state. Titchener's[11] formulation of attention in terms of attributive clearness was followed by experiments carrying his basic assumptions. The year after the publication of Titchener's book Geissler[12] brought forward an experiment on the relation of attributive clearness to efficiency of behavior, based on visual processes. Four years later the work was carried forward in a similar way by Dallenbach[13] who attempted to correlate attributive clearness of auditory states with efficiency of behavior. He gave a correlation (product-moment method) of .85 as the lowest result when comparing efficiency of work performed with verbal reports on clearness.

Dallenbach says, "Previous experimenters have found that distractors very soon lose their power of compelling the attention; the observers become habituated." Here we have a direct allusion to the fact that resistance to a distraction can become automatized. Here also we seem to see, for the first time, a real attempt to control the problem, for Dallenbach tried to keep the distractions constantly varied in order that 'habituation' might be kept at a minimum. Whether he succeeded or not could only have been told by some sort of a temporal curve, showing the changes in efficiency as the various distractions continued. In all probability, even a variable distraction can ultimately be resisted. But it is a very significant fact that Dallenbach reversed the opinions of previous investigators in the statement that the distractions impaired efficiency instead of increasing it. It would seem that this conclusion would not have been possible had he not been very careful in controlling the fact of 'habituation.' We need not quibble concerning Dallenbach's method of securing evidence of attention. Morgan's substitution of a direct measurement of overt behavior for the verbal report or 'introspective report' has nothing to do with the principle. Since the correlation between the two types of behavior is high we may regard either as an adequate measurement of attention. The fact is, we seem to have here evidence that Dallenbach measured the *initial* effects of distraction, while previous experimenters had been measuring *cumulative* effects and securing, therefore, opposite and contrasting results.

It is evident that so far there were no attempts to show, by means of a smooth curve, how distractions can affect efficiency of behavior as time passes. Bits of incidental evidence occasionally were given, but not with the motive of showing the process nature of the resistance to distraction. Indications of the principle may be noted in some of the studies on the curve of efficiency for the acquisition of skill. Batson[14] noted that a distraction was more effective in the beginning than toward the end of the acquisition of a skilled performance. He held that the effect of a distraction is never totally lost, a statement that seems to indicate that we are dealing only with an approach to limits—a psychological application of the notion of relativity. Some of the experiments on reaction-time under the influence of distraction remind one of the days of the older Wundtian laboratory and the warning of Cattell on the importance of automatization. The contribution of Evans[15] on the relation of distraction to reaction-time is an example. Evans used light, sound, and touch distractions in simple reaction-time experiments and obtained temporal curves which indicated a definite gradient. The repetition of a simple reaction implies a fairly rapid automatization rate and would not be regarded by all as a good example of the effect of distraction on attention. As a matter of fact, we see no reason why Evans' curves may not be regarded as a measure of the attention-automatization gradient.

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[11] E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, Lect. i, v, and vii.

[12] L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 473-529.

[13] K. M. Dallenbach, The measurement of attention, this JOURNAL, 24, 1913, 465-507. Cf. also The measurement of attention in the field of cutaneous sensation, *ibid.*, 27, 1916, 443-460.

Dissatisfied with Dallenbach's measurement of attention by introducing verbal reports on attributive clearness, Morgan[16] decided to carry forward a distraction experiment in which all computations should be based on immediate and direct behavioristic measurements. Because he apparently felt that there was no safe criterion as to what constituted attention in terms of behavior, Morgan seems to have decided definitely to avoid the word *attention*, allowing his readers to make whatever inferences were desired as to the principles of mental control behind behavior. His experiment is generally classified as one on attention, from its apparent resemblance to traditional experiments.

Morgan found that the average reaction-time for the solution of difficult problems is less under noise than under quiet conditions. He also found that the first problems in a series under distraction require a longer time for solution than later ones. The increase in speed for the later problems more than compensated for the slowness in working the initial problems of the series. His method of averaging together ten or more problems concealed the possibility of a smooth curve of distraction. The fact that he did not average the entire period together was the saving point in his technique of computation; it left something for the process nature of adaptation to distraction. He measured the concomitant motor effects by means of key pressure and found that the increase of efficiency was accompanied by a dynamogenetic effect of distraction. Although Morgan's statement that distraction increased efficiency as measured by speed, we must remember that this statement is the result of averaging together several problems in the same distraction period and the statement means just what the older statements meant, *i.e.* the cumulative effects of distraction, or automatized resistance to distraction accompanied by increased incentives to work, not the initial effects of distraction on attention as measured by

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[14] W. H. Batson, Acquisition of skill, *Psychol. Monog.*, 21, 1916, (no. 91), 86.

[15] J. E. Evans, The effect of distraction on reaction-time, *Arch. Psychol.*, 37, 1916.

[16] Morgan, *op. cit.*

Dallenbach. The fact that Morgan did take into account the difference between initial and cumulative effects of distraction and mentioned the difference is the saving point in his work which suggests the possibility of a point of view leading to a definition of attention and automatization as extremes of a gradient process. Morgan used noises for distraction, which were unique in that they were unusually severe. The same type of noise was used throughout one sitting.

The precision of Morgan's experiment places it in a position of importance in solving the problems relating to the nature of attention. Direct experiments on the problem have not been attempted in the European laboratories; but there are some researches which seem to contribute interesting possibilities of explanation. Claparède,[17] for instance, had begun an experiment on fatigue which resolved itself later into an experiment on attention. When a subject was given the task of contracting both hands simultaneously on a pair of ergographs until paralysis resulted, the cessation of *trying* to contract one hand was accompanied by a resumption of actual movement of the other hand although no rest-pause had been given. Now the sudden resumption of movement by a member that was previously paralyzed with fatigue seems to offer something important in the nature of the central controls of volition. Claparède suggested an explanation in terms of the drainage of energy from central reservoirs which had been divided while innervating two hands, but which are later allowed to throw their entire force into one hand. In terms of behavior, we know that there was present an interference of efficiency when two hands were used, and it would seem that the postulation of reservoirs carries us beyond scientific necessity.

An extension of Claparède's work was accomplished by one of his pupils, Rimathé,[18] using a similar ergograph, but so arranged that homolateral pairs of muscles could be used. He found that the same principles held for muscles on the same side of the body, an indication that the phenomenon could not be correlated with the paired functions of the cerebral hemispheres.

The fact seems to be, however, that we have here what is essentially a distraction experiment. Verbal reports on the part of subjects in such fatigue experiments indicate that the process of producing fatigue-paralysis is accompanied by exceedingly uncomfortable feeling from the members being used and that there were changes in incentives when the fatigue pains were removed from one of the hands—enough to account for the increase of efficiency. While Morgan did not carry his experiments far enough to produce a great amount of sensible fatigue, we may indicate here that in all probability we must be on our guard against the inhibiting effects of proprioceptive as well as exteroceptive stimuli, and regard them as possibilities of distraction.

III. APPARATUS AND PROCEDURE

With the foregoing criticisms in mind, we wished to show the changes in efficiency for each successive problem carried on without disrupting the temporal order, both with and without distraction. At the same time we wished to make the period of work short enough to reduce the chance of extreme automatization of the problems. It will be seen that, instead of eliminating the practice curve, we have deliberately kept our observations within its scope. Efficiency of behavior will be measured in terms of immediate and direct performance, with enough verbal report allowed to throw more light on the causes of irregularities. In order that the cumulative effect of distraction might be taken into account we decided to average together all first problems apart from the average of all second problems of the series, and these in turn apart from all third problems, and so on. Our graphs will represent the function of reaction-time plotted against position in the series.

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[17] E. Claparède, L'ergographie bilatérale, *Arch. des sci. phys. et nat.*, 44, 1917, 71.

[18] F. Rimathé, Contribution à l'étude de l'ergographie bilatérale et simultanée, *Arch. de psychol.*, 19, 1924, 128-162.

In spite of the fact that Morgan's problems were much better than those used in many other experiments, in that they were much more difficult and therefore less rapidly automatized, we believed that it would be possible to frame problems which were equally difficult and which at the same time placed a penalty on any fluctuations of attention from the work at hand. Any deviation from the following problems will endanger forgetting the sum which *S* is carrying and produce a corresponding long reaction brought about by the necessity of going back to the beginning of a problem, or there may be a false answer.

Ordinarily arithmetical operations may be automatized as generalized pattern-situations; so if they are to be used they must be placed in unusual combinations or environments. We framed 60 problems involving the selection of single digit numbers from a row of mixed letters and numerals, *S* being told to add these numbers as he discovered them and to write their sum on a strip of paper. The following is an example of one of the problems:

Gd7Fc8NfEaW9MBcOPT5FzA4NvCXz6MkhgP9xtb.

The numerals were scattered throughout the series of symbols in no predictable order either as to spacing or kind. The symbols were secured by chance as was also the spacing of the various digits.

The accumulation of muscular tonus during the distraction was an important point in Morgan's work and was made an index to the intensity of the mental process. We had no objection to Morgan's method of denoting the presence of the factor and had no doubts concerning his statements of the results. Because our *Ss* wrote their answers on paper instead of pressing keys, we used writing pressure instead of key pressure, indicating the amount by means of a tambour recorder on a strip of kymographic paper. The same pencil was used throughout all runs of the experiment in order that there might not be variations of behavior due to differences in hardness of the writing point.

Morgan's distractions, as we have indicated, were more than usually severe and we agreed on the whole with his selection. We had but one difficulty in selecting the noise to be used, the matter of the difference between monotonous or constant noises and variable noises. If we used the evidence based on a comparison of Morgan's and Dallenbach's results, the former would produce rapid and the latter slower automatization. We did not think it necessary to use the great variety of noises introduced by Morgan, since many of them must have given identical results.

Our choice finally fell on two methods of creating a noise distraction, a Klaxon automobile horn placed about two feet from *S*'s ear, and a phonograph operated with a loud needle, the amplifying horn being adjusted as closely as possible to *S*'s head. A monologue in a male voice delivered its supposedly humorous story into the ear of *S* while he was trying to solve the problems outlined above. In the preliminary experiment the phonograph was used entirely and in the later experiments we used the automobile horn.

The room in which *S* sat was illuminated entirely by artificial light, the walls being painted black and free from all light-distracting objects. The experiment was conducted in a section of the building which was relatively free from extraneous and uncontrolled noises. A 60-w. lamp overhead gave a small amount of illumination all the time, while another 60-w. lamp on the table at *S*'s side was so screened that it threw a bright light on the card of problems held in front by a bracket. The table lamp was so connected that *E* could, without entering the room, throw it on as a signal to begin work and turn it off as a signal to stop. *S* was always alone in the room while doing the problems; the kymograph, recording instruments, motor and control switches were all in another room, sound-insulated from him.

The apparatus on *S*'s side of the partition consisted of the following items. An endless belt, for exposing the code numbers indicating which problem was to be solved, was arranged in such a manner that the pushing of a lever turned a new code-stimulus into view and at the same time drew a fresh space on the strip of white paper used for recording the answers. The strip of paper on which the answers were written was drawn on a spindle so as to run over a large tambour on which very stiff rubber had been stretched and then covered with a wooden plaque. The tambour thus constituted an indicator of writing pressure. The amount of deflection of the tambour was imperceptible to the *S*s, only four of whom knew that the writing-pressure was being recorded. A tube extended through the wall from the writing-pressure board to the tambour-recorder on the kymograph. The tension of the writ ng-pressure board was calibrated by placing weights on the paper and noting the amount of deflection of the recording needle.

A Lombard pneumograph was strapped by means of a broad leather band to the upper portion of the thorax and was used for the same purpose as that used by Morgan, *i.e.* to secure indications of articulation. Since blood circulation was also being measured the respiratory changes constituted one of the checks on vaso-motor activity. The left fore-finger and second finger were pressed firmly to a rubber bulb and fastened to it by means of adhesive tape. A tube led from the bulb to the piston-recorder, the usual arrangement for the

Lombard-Pillsbury plethysmograph. The arm of *S* was now suspended in a sling hung from the ceiling in order to allow for body-movements without interfering with the operation of the plethysmograph. It was hoped that this portion of the apparatus might produce evidence in addition to Morgan's statements on articulation and key-pressure.

The kymograph used was developed in our laboratory.[19] It employed high tension discharge of electric sparks as a means of making a graphic record on white paper. The graphic record consisted of the following lines. (1) The perforated line of blood-volume taken with the piston-recorder. (2) The time-line (in sec.) taken with period perforations controlled by connecting the spark discharge in series with a mercury-contact seconds-clock. Some of the records were taken with the kymograph being driven by a synchronous motor operating on 60-cycles current from the power-house. Checks were made frequently for variations of speed, but as the variability of the motor was less than 1% of the variability of the data, there seemed to be no reason for rejecting the method of measuring time in terms of relative linear distances on the kymograph record. (3) A perforated line from the needle of a tambour giving a record of breathing changes. (4) A perforated line controlled by *E*'s switch indicating the exact moments the distraction was turned on and off. 5) An ink-needle tambour giving a record of the amount of writing-pressure exerted by *S*, and also a means of ascertaining the writing-time which was not counted with the total reaction-time.

At the beginning of the first sitting, *E* told *S* the nature of the problems and worked one out, in order that the procedure might be perfectly clear. The noise was turned on for a moment in order that *S* might become acquainted with the nature of the distraction. This appeared necessary, especially with the automobile horn, because of the tendency of the *S*s to jump from their chairs if the distraction were turned on without previous warning. *S* was told then to start working just as soon as the lamp on the table lighted, to work as rapidly as he could, to remember that both speed and accuracy records were being taken, and to permit no interference from anything until the light on the table was extinguished.

S worked upon not more than 8 of the problems, when the distraction was turned on at exactly the beginning of a problem. Thus the preliminary quiet period was, on an average, about a minute-and-a-quarter long. The distraction remained on for exactly 6 problems, then it was abruptly discontinued. Then there followed 6 or more problems worked in absolute quiet. The 6 problems before the distraction, the 6 during the distraction and the 6 after the distraction were measured and these measurements given a place in frequency curves, a curve being made for each position in the series. Although there were often as many as 8 problems before the distraction, only the 6 preceding the distraction were tabulated. We varied the number of problems in the first and third periods in order to prevent anticipation of the end of the period by the *S*s. In the first period this prevented fear of the distraction affecting the final problem, and in the third prevented the possibility of the 'end spurt.' It is true that the working period in our experiment was much shorter than in Morgan's. Some of the *S*s complained that they had scarcely got 'warmed up' to the work when they were forced to stop. But it must be remembered that we were working under the assumption of the possibility that excessive training removes attention, that the best place for measurement of attention is in the initial stages of the practice curve.

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[19] Adelbert Ford, Recording apparatus: the electrokymograph, *J. Exper. Psychol.*, 7, 1924, 157-165.

After each sitting *S* was asked to describe what happened while he worked. Since the results of the experiment were to depend heavily on the efficiency of performance, this 'introspection' was introduced merely to secure evidence concerning any unpredictable conditions. After they had finished their voluntary description they were asked a few direct questions: "Did you have any emotion?" "Did you lose your place while working?" "What happened when the noise stopped?" "Have you ever had experience in an occupation which required a great amount of arithmetical addition?" "What effect do you think the noise had on your speed and efficiency?" "Did you have any difficulty with the manipulation of the apparatus?" They were also asked to make statements concerning health, age, state of rest or fatigue, and mood.

During the first run there were probably other distractive conditions than the noise itself. The *S*s were not accustomed to the conditions of the psychological laboratory. Many were more or less nervous upon entering and being seated in an eerie, black room, knowing that "something in their mind was being measured." The experience of being strapped into various instruments was another source of novel stimulus which was certain to bring about changed attitudes toward the work at hand. But there was strong evidence that these uncontrolled environmental and attitudinal effects rapidly disappeared. Several of the *S*s, forgetting that they had a bulb in their fingers and a pneumograph on their chests, walked off as soon as the experiment ended, tearing the instruments from their respective tubes. The nervousness from the fear of being "mentally measured" likewise disappeared after the first run and they reported taking the experiment as a matter of course. All but three of the *S*s were undergraduate students in elementary psychology. We have since regretted that we did not give every *S*, before the principal experiment, a period of special work to produce environmental orientation. From the point of view we have taken here, this would mean "automatization to to uncontrolled distractors." In all probability our *S*s did not get accustomed to the laboratory environment until the second sitting of the experiment.

It may be said that the introduction of an automobile horn as a distraction was likely to produce emotional effects which constituted an unmeasured variable. This criticism cannot be answered in any form of distraction experiment. There are always emotional and feeling effects and they cannot be totally eliminated. It may even be possible that the situation which brings about new integrations necessitating strong dominance, *i.e.* attention, involves the very conditions which make emotions unavoidable. We are not at all certain that in all situations where one pattern opposes another the emotion is not the working principle of the distracting effect.

IV. The Preliminary Experiment

The first runs in the experiment were started in the fall of 1921 and were made largely with a view toward perfecting the apparatus. Apparently the phonograph did not give nearly as much of an emotional shock as did the automobile horn. While we had thought that the phonograph might constitute a variable distraction, and so prevent automatization, we found that most of the *Ss* reported that they heard nothing from the instrument that was more than vaguely meaningful, there being only an occasional awareness, confused and fragmentary, of a male voice talking in strident tones. This seemed to have reduced the effectiveness of the phonograph to that of a monotonous distractor, much inferior to the horn. In the later experiments we abandoned the phonograph.

TABLE I

Preliminary Experiment

Measurements of time (sec.) required for the solution of problems. An experimental period consisted of 18 consecutive problems: 6 under quiet conditions, 6 under distraction (phonograph), and 6 under quiet conditions again. Based on frequency curves of 68 experimental periods

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.	Errors
11.847	14.733	16.176	4.794	11.382	20.970	11
12.176	15.000	16.412	4.457	11.955	20.869	6
13.494	15.400	16.353	4.467	11.886	20.820	7
13.851	15.833	16.824	4.650	12.174	21.474	15
13.193	15.429	16.412	4.747	11.665	21.159	14
13.294	16.000	17.353	4.492	12.861	21.845	11
16.273	18.091	19.000	5.149	13.851	24.149	11
14.352	16.000	16.824	4.269	12.555	21.093	12
19.811	18.077	17.210	3.641	13.569	20.851	14
14.381	16.833	18.059	4.704	13.355	22.763	10
15.811	17.545	18.412	4.808	13.604	23.220	6
13.823	16.333	17.588	4.375	13.213	21.963	16
15.624	16.600	17.088	3.845	13.243	20.933	16
15.014	15.750	16.118	3.659	12.459	19.777	12
13.880	16.058	17.147	3.784	13.363	20.931	7
14.732	16.636	17.588	3.975	13.613	21.563	15
15.051	16.429	17.118	4.014	13.104	21.132	12
12.737	15.363	16.676	3.973	12.703	20.649	10

The figures in Table I give the results of 68 experimental runs, an average of four from each person, in terms of the computed mode, median, mean, and mean deviation, and the number of errors committed out of 68 problems, the phonograph being used as a distraction. We used the mean as a measurement of the central tendency throughout all work. The median and com-

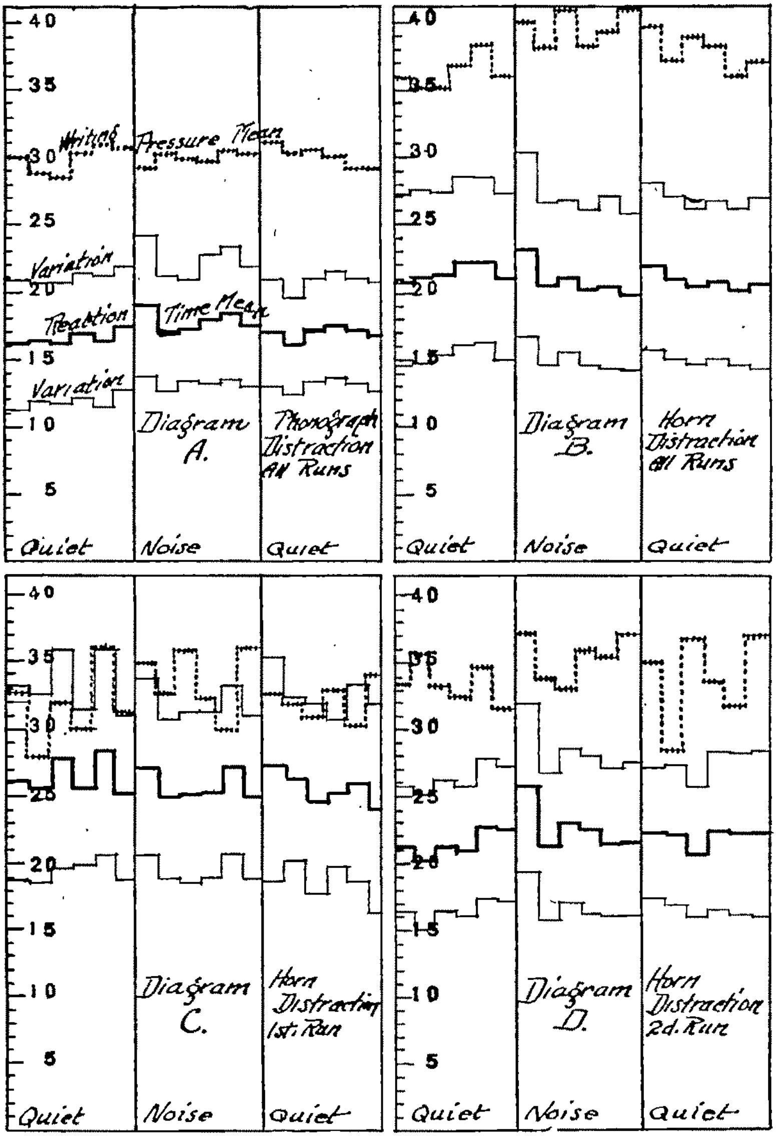
40
35
30
25
20
15
10
5
Writing Pressure Mean
Variation
Reaction Time Mean
Variation
Diagram A.
Phonograph Distraction All Runs
Quiet
Noise
Quiet
Diagram B.
Horn Distraction All Runs
Quiet
Noise
Quiet
Diagram C.
Horn Distraction 1st Run
Quiet
Noise
Quiet
Diagram D.
Horn Distraction 2d Run
Quiet
Noise
Quiet

FIG. 1

puted mode were given for reasons to be considered later. Diagram A (Fig. 1) shows the facts of the table in graphic form wherein the vertical coördinate represents reaction-time and the horizontal coördinate represents the series of problems worked. The levelness of the reaction-time measurements for the first 6 problems in all the graphs rather gives one the notion that the slope of the curve of practice is very gradual. The values in all tables for every problem in the series are based on averages of 38 runs or more. The headings of the tables give the number of measurements averaged together.

The following points are to be noted in the curve of means for Table I, based on 68 runs, with the phonograph as a distraction:

(1) The practice effect is not present.

(2) The initial problem in the distraction period displays a conspicuously greater reaction-time than any other in the series. The entire 6 problems of the distraction period are longer than those of the quiet periods.

(3) There is no evidence of an initial long reaction-time for the last quiet period.

(4) There is a very poor correlation between writing-pressure and the reaction-time.

V. METHODS OF COMPUTATION

We have given the mean deviation in our tables for the purpose of indicating any changes in the variability of efficiency under various conditions. The fine lines on the diagrams show the limits of mean deviation on each side of the mean. The causes of deviation are important for consideration, although to some extent problematical. We should probably have to consider differences in *ability* in this type of arithmetical work, differences in the *difficulty* of the problems being used, changes in the *attitude* of the subjects from problem to problem, *fatigue* effects, *minor distractions* from discomfort and other organic conditions, as well as other factors impossible to identify.

Practically all of the frequency curves secured for every problem in the series show evidence of skewness. The measurement for the skewness of a curve was suggested by Pearson, and we have given enough of the curve measurements to enable computation of this factor. Pearson had noted that in most skewed curves the distance of the mode from the mean was three times that of the distance of the median from the mean. This gave him a means of computing the mode, given the mean and the median, with but small error from the position of the observational mode. Our use of the computed mode in our tables is presented solely as a means of denoting the relative amount of skewness. Pearson's suggestion for a formula for skewness which would not be expressed in original units was

$$\text{Skewness} = \frac{\text{Mean} - 3(\text{Mean} - \text{Median})}{\text{Standard Deviation}},$$

but there is no essential reason for abandoning the original units as long as we make comparisons in homogeneous data. Therefore it seems that the difference between the mean and the median may be taken in our tables as an indication of the skewness.

The meaning of skewness must affect our interpretation of results. Whenever the mean shows a definite change, either higher or lower, we must know whether the change was caused by a few extreme and exceptional cases or was brought about by a tendency for a small amount of change in all cases. If the distraction, for instance, should cause an *S* to forget his accumulated sum, he might occasionally have to go back and pick up his work from the beginning of that particular problem. This might not occur often; but such a condition would have a tendency to add cases on the end of the curve already having the broader range and increasing the skewness more than ever. On the other hand, if the distraction has the effect of destroying automatization and *S* is merely displaying a slower associative tendency, we might expect all the reactions to be longer by a fairly constant increment. It is evident that the increase in skewness would be accompanied by a small increase in the mean deviation. If the distraction had the greater effect on the shorter reactions, *i.e.* the ones which were successfully completed without the subject losing his place, then the mode would be thrown farther away from the negative end of the distribution and the amount of skewness would be seen to decrease.

The amount of skewness in Table I decreases the moment *S* begins to work under distraction. The mode is thrown nearer the positive end of the distribution, an indication that the effect of the distraction is probably general on all reactions and not just a pronounced effect of a few reactions which are the result of *S* losing his sum and having to go back to the beginning of the problem.

Although our *S*s showed a greater tendency to commit errors than did Morgan's, there seems to be no indication of a correlation between number of errors and the presence of distraction. It is true that there are more errors in the first half of the distraction-period than in the last half; but in view of the great variability of errors from time to time it seems that we cannot be sure that this is not coincidence.

In the experiments where we used the phonograph as a distractor there seemed to be very little evidence of any correlation between the writing-pressure used by the *S*s and the length of time for working out the problems, and no observable transition in writing-pressure at the time the distraction appeared. We are inclined to believe that this failure to get results was due to the weakness of the distraction used.

Reports by the *Ss* seemed to give some clues as to mental attitude.

Most *Ss* reported annoyance at the beginning of the noise-period and relief at the beginning of the following quiet-period. Nearly all were able to state that they were retarded at the beginning of the noise-period, but only 4 that they recovered speed as the noise-period progressed. None of the *Ss* were able to tell the story given in the monologue even after the fourth repetition of the same record. One *S*, a graduate student in psychology, reported after the first run that she had not heard the record at all and did not know that the phonograph had been turned on. She apparently received no effect at all from the distraction even by measurement of the reaction-times. No other *S* ever reported complete obliteration of the distraction. Five of the 17 *Ss* in this first group reported that they customarily studied with a phonograph playing in their rooms, a temporary fad among students in the University at that time. The composite curve of these 5 *Ss* showed such great variability that we cannot state the influence of the previous training.

VI. Later Experiments

Whereas in the preliminary experiment with the phonographic distraction we had used 16 *Ss*, in the fall of 1922 we arranged to use 41, 38 undergraduates and 3 graduates in psychology. We corrected the apparatus in such a manner as to simplify *S*'s work in changing problem-cues, and to have a finer calibration to the writing-pressure board. The same problem card was used as before. We moved the spark commutator into a distant room to prevent the least possible noise, and used a quiet synchronous motor for moving the kymographic mechanism, measuring time by relative distances on the kymograph-paper. We substituted the automobile horn for the phonograph.

Table II gives the results of 183 sittings from 41 *Ss*. Diagram B (Fig. 1) shows the results in graphic form. Table VI shows the measurements of writing pressure in these same sittings. Table III is a composite result of first sittings for 38 *Ss*, the first run only. Diagram C (Fig 1) gives the graphic illustration. Table VII gives the writing-pressure measurements for Table III. Table IV and Diagram D (Fig. 1) give the results of the second consecutive sitting for the same 38 *Ss* and Table VIII gives the corresponding

TABLE II

Time Measurements

From frequency curves of 183 experimental periods

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.	Errors
15.621	19.167	20.940	6.259	14.681	27.199	47
13.463	18.579	21.137	6.425	14.712	27.562	41
16.751	19.857	21.410	6.036	15.374	27.446	34
13.359	19.273	22.230	6.179	16.051	28.409	34
18.061	20.833	22.219	6.150	16.069	28.369	40
17.253	19.769	21.027	6.123	14.904	27.150	34
18.947	21.667	23.027	6.250	16.777	30.277	34
15.741	18.923	20.514	5.899	14.615	26.413	52
16.659	19.571	21.027	5.613	15.414	26.640	45
17.152	19.320	20.404	5.669	14.735	26.073	30
15.983	19.091	20.645	6.235	14.410	26.880	31
13.067	17.667	19.967	5.737	14.230	25.704	45
18.402	20.684	21.825	6.211	15.614	28.036	43
16.339	19.421	20.962	6.016	14.946	26.978	40
15.712	18.840	20.404	5.704	14.700	26.108	41
15.249	18.933	20.775	5.856	14.919	26.631	42
18.249	19.555	20.208	5.788	14.420	25.996	35
15.103	18.739	20.557	6.291	14.266	26.848	32

TABLE III

Time Measurements

The work of 38 *Ss* without previous training

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.	Errors
27.181	26.429	26.053	7.216	18.837	33.269	8
22.642	24.600	25.579	6.820	18.759	32.399	4
20.315	25.333	27.842	8.141	19.701	35.983	5
23.842	25.000	25.579	5.684	19.895	31.263	11
26.078	27.500	28.211	7.612	20.599	35.823	13
23.484	24.600	25.158	5.745	19.413	30.903	6
22.998	25.666	27.000	6.684	20.316	33.684	9
20.704	23.500	24.895	5.983	18.912	30.878	11
22.706	24.200	24.947	6.310	18.637	31.257	11
21.790	24.000	25.105	6.111	18.994	31.216	8
27.000	27.000	27.000	6.263	20.737	33.263	7
25.210	25.000	24.895	5.947	18.948	30.842	12
20.898	25.000	27.052	8.291	18.761	35.343	11
25.686	26.000	26.157	6.174	19.983	32.331	13
25.422	25.000	24.789	7.105	17.684	31.894	11
23.484	24.600	25.158	5.640	19.518	30.798	12
19.314	26.666	25.842	7.291	18.551	33.133	9
21.106	23.000	23.947	7.896	16.051	31.843	8

TABLE IV

TIME MEASUREMENTS

The work of 38 *Ss* one week after the first run tabulated in Table III

Mode	Median	Mean	M.D.	M. – M.D.	M. + M.D.	Errors
18.015	20.111	21.159	4.455	16.704	25.614	10
18.794	19.666	20.102	5.064	15.038	25.166	12
22.629	21.859	21.474	4.728	16.746	26.202	4
21.106	21.000	20.947	4.947	16.000	25.894	4
16.986	20.750	22.632	5.152	17.480	27.784	6
19.868	21.500	22.316	4.997	17.319	27.313	8
17.842	23.000	25.579	6.324	19.255	31.903	4
16.394	19.500	21.053	5.535	15.518	26.588	12
19.776	21.750	22.737	5.773	16.964	28.510	14
14.800	19.600	22.000	5.895	16.105	27.895	1
18.893	20.333	21.053	5.058	15.995	26.111	5
18.761	20.429	21.263	5.186	16.077	26.449	9
19.974	21.500	22.263	4.936	17.327	27.199	9
20.998	21.666	22.000	5.263	16.737	27.263	7
17.946	19.666	20.526	4.792	15.734	25.318	10
18.474	21.000	22.263	5.883	16.380	28.146	5
14.394	19.500	22.053	5.961	16.092	28.014	10
18.894	21.000	22.053	6.072	15.981	28.125	5

TABLE V

PRELIMINARY EXPERIMENT

Measurement of writing pressure while writing the answers to problems. An experimental period consisted of 18 consecutive problems: 6 under quiet conditions, 6 under distraction (phonograph) and 6 under quiet conditions again. Based on frequency curves of 68 experimental periods

Mode	Median	Mean	M.D.	M. – M.D.	M. + M.D.
9.840	10.000	10.080	2.341	7.739	12.421
8.871	9.375	9.627	2.340	7.287	11.967
10.071	9.735	9.567	2.250	7.317	11.817
10.092	10.100	10.104	2.280	7.824	12.384
7.601	9.409	10.313	2.650	7.663	12.963
8.172	9.500	10.164	2.700	7.464	12.864
9.906	9.750	9.672	2.562	7.110	12.234
8.898	9.722	10.134	2.489	7.645	12.623
11.305	10.375	9.910	2.289	7.621	12.199
10.508	10.100	9.896	2.393	7.503	12.289
10.041	10.083	10.104	2.304	7.800	12.408
9.381	9.863	10.104	2.453	7.651	12.557
11.331	10.643	10.299	2.972	7.327	13.271
10.123	10.071	10.045	2.737	7.308	12.782
8.580	9.666	10.209	2.864	7.345	13.073
10.063	10.071	10.075	2.618	7.457	12.693
9.008	9.500	9.746	2.556	7.190	12.302
9.527	9.643	9.701	2.581	7.120	12.282

TABLE VI

WRITING-PRESSURE MEASUREMENTS

The following values are based on frequency curves of 183 experimental periods

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.
5.849	7.921	8.957	3.614	5.343	12.571
6.575	7.974	8.768	3.286	5.482	12.054
6.111	7.853	8.724	3.424	5.300	12.148
6.412	8.310	9.259	3.622	5.637	12.881
6.604	8.630	9.643	3.934	5.709	13.577
5.927	8.033	9.086	3.757	5.329	12.843
6.881	9.011	10.076	4.029	6.047	14.105
7.295	8.763	9.497	3.848	5.649	13.345
7.909	9.447	10.216	3.871	6.345	14.087
7.559	8.909	9.584	3.942	5.642	13.526
7.418	9.024	9.827	3.788	6.039	13.615
7.190	9.312	10.373	3.984	6.389	14.357
7.893	9.283	9.978	3.555	6.423	13.533
7.333	8.563	9.178	3.754	5.424	12.932
7.992	9.100	9.654	3.543	6.111	13.197
7.222	8.786	9.568	3.690	5.878	13.258
6.835	8.329	9.076	3.520	5.556	12.596
6.796	8.442	9.265	3.750	5.515	13.015

TABLE VII

WRITING-PRESSURE MEASUREMENTS

The following values are taken from the work of 38 *Ss* without previous training

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.
5.217	7.125	8.079	2.472	5.607	10.551
4.816	6.500	7.342	2.801	4.541	10.143
6.552	7.500	7.974	2.079	5.895	10.053
5.948	7.000	7.526	2.213	5.313	9.739
5.948	8.000	9.026	3.561	5.465	12.587
6.268	7.300	7.816	2.860	4.956	10.676
8.078	8.500	8.711	2.711	6.000	11.422
8.036	8.100	8.132	2.514	5.618	10.646
9.458	9.100	8.921	2.249	5.883	10.381
9.611	8.625	8.132	2.388	5.744	10.520
10.552	8.500	7.474	2.210	5.264	9.684
8.115	8.091	9.079	2.091	5.988	10.170
7.226	7.900	8.237	2.643	5.594	10.880
6.658	7.500	7.921	2.500	5.421	10.421
7.132	7.500	7.684	2.368	5.316	10.052
7.422	8.000	8.289	2.305	5.984	10.594
7.394	7.500	7.553	2.132	5.421	9.685
9.410	8.750	8.420	2.450	5.790	10.870

TABLE VIII

Writing-Pressure Measurements

The following values are taken from the work of 38 *Ss* one week after the first run tabulated in Table VIII

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.
4.423	7.071	8.395	3.499	4.896	11.894
5.208	7.666	8.895	3.446	5.449	12.341
6.763	7.833	8.368	3.091	5.277	11.459
5.288	7.166	8.105	2.402	5.703	10.507
7.328	8.250	8.711	3.169	5.542	11.880
6.606	7.500	7.947	2.789	5.158	10.736
7.314	8.666	9.342	3.537	6.805	12.879
5.342	7.500	8.579	2.828	5.751	11.407
7.106	8.000	8.447	2.997	5.450	11.444
5.447	7.833	9.026	3.666	5.360	12.692
7.606	8.500	8.947	3.474	5.473	12.421
4.468	7.700	9.316	3.715	5.601	13.031
7.322	8.300	8.789	3.094	5.695	11.883
6.342	6.900	7.179	2.660	4.519	9.839
8.611	9.100	9.263	3.515	5.748	12.778
4.694	7.214	8.474	3.388	5.086	11.862
4.258	6.700	7.921	2.957	4.964	10.878
4.500	6.500	9.316	5.256	4.060	14.572

TABLE IX

Time Measurements

The following values are taken from the work of 38 *Ss* two weeks after the first run tabulated in Table III and one week after the second run tabulated in Table IV

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.	Errors
16.626	18.454	19.368	4.637	14.731	24.005	7
15.448	16.500	17.026	4.738	12.288	21.764	13
16.965	18.111	18.684	3.512	15.172	22.196	9
12.368	17.000	19.316	5.277	14.039	24.593	7
21.842	21.000	20.579	4.368	16.211	24.947	4
13.684	17.000	18.658	5.296	13.362	23.954	8
20.448	20.500	20.526	5.291	15.235	25.817	9
16.000	18.000	19.000	4.737	14.263	23.737	12
16.684	19.000	20.158	5.244	14.914	25.402	5
17.336	18.200	18.632	4.067	14.565	22.699	9
14.842	18.000	19.579	4.407	15.172	23.986	5
14.368	17.000	18.316	4.715	13.601	23.031	11
20.684	21.000	21.158	5.684	15.474	26.842	9
17.790	18.000	18.105	4.280	13.825	22.385	5
18.052	19.000	19.474	5.000	14.474	24.474	5
15.294	17.800	19.053	4.798	14.255	23.851	11
17.106	19.000	19.947	4.789	15.158	24.736	7
17.682	18.666	19.158	4.737	14.421	23.895	5

TABLE X

Writing-Pressure Measurements

The following values are taken from the work of 38 *Ss* two weeks after the first run tabulated in Table VII and one week after the second run tabulated in Table VIII

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.
6.175	7.725	8.500	3.765	4.737	12.263
5.184	7.500	8.658	3.235	5.423	11.893
4.937	7.277	8.443	3.149	5.298	11.596
7.735	8.333	8.632	4.519	4.113	13.151
5.328	7.250	8.211	3.285	4.926	11.496
7.657	8.833	9.421	3.720	5.701	13.141
6.434	8.2500	9.158	3.400	5.758	12.558
5.750	8.250	9.500	4.158	5.342	13.658
8.526	9.000	9.237	3.525	5.712	12.762
8.076	9.166	9.711	4.289	5.422	14.000
5.658	8.500	9.921	4.334	5.587	14.255
7.815	9.833	10.842	4.247	6.595	15.089
8.342	9.500	10.079	2.999	7.080	13.078
8.268	9.300	9.816	3.753	6.063	13.569
9.420	9.666	9.789	3.695	6.121	13.511
7.342	8.500	9.079	3.525	5.554	12.604
7.078	8.500	9.211	3.896	5.315	13.107
7.09	8.100	8.605	3.090	5.515	11.695

writing-pressure values. Table IX and Diagram E (Fig. 2) give the results of the third consecutive run for the same 38 *Ss*, the runs being one week apart, and Table X gives the corresponding writing-pressure measurements. Diagram F shows the reaction-times for the 20 *Ss* who were the most rapid in addition of the digits, and Diagram G shows the reaction-times for the 21 *Ss* who were the slowest in ability in the problems. In these latter two diagrams the reaction-times have been multiplied by a factor to reduce their curves approximately to the same level. Diagram H represents the author's conception of the effect of distraction.

Evidence of automatization of problem-solving does not seem to be present in a single sitting. There is no increase in the speed of working in the first 6 problems of the 'normal' period; and there is no appearance of a lowering of the level of the means in the entire run anywhere. But when we examine Tables III, IV and IX and Diagrams C, D, and E (Figs. 1, 2) we note that in the successive runs for the same *Ss* there is a distinct increase in the speed of working the problems, accompanied by a distinct decrease in the number of errors. It appears that the lapse of a week between sittings seems to bring out a tendency toward auto-

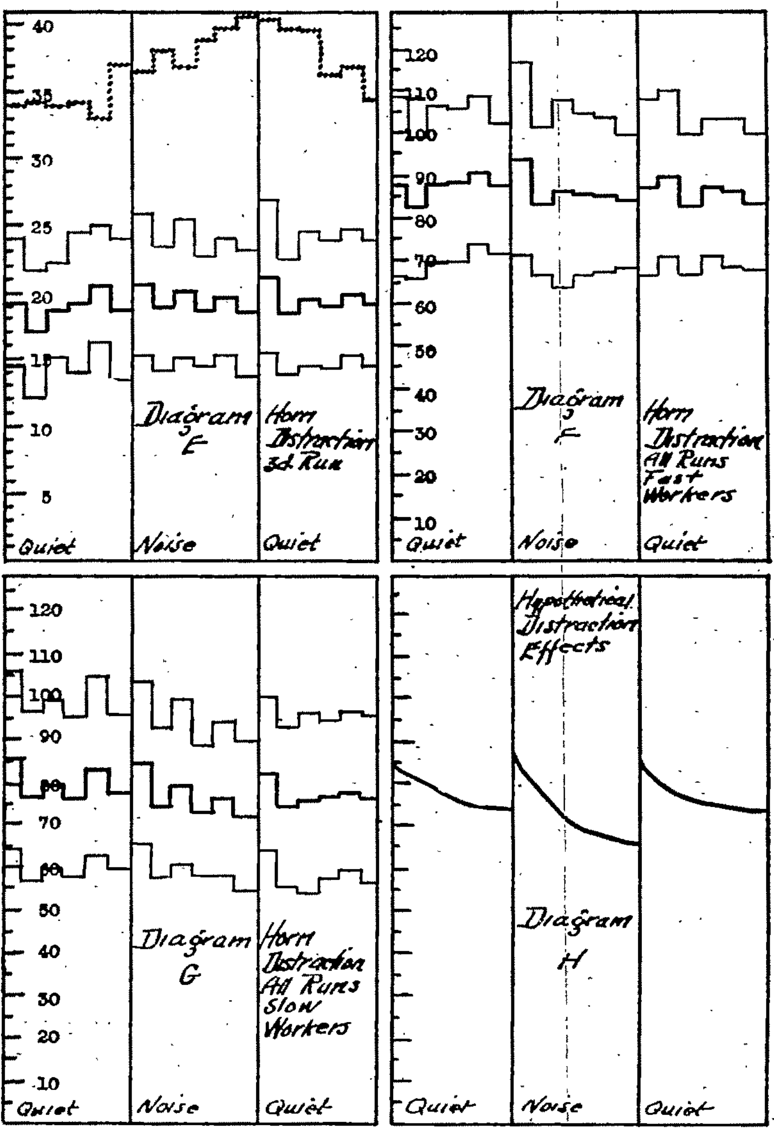
40
35
30
25
20
15
10
5
Diagram E
Horn Distraction 3d Run
Quiet
Noise
Quiet
120
110
100
90
80
70
60
50
40
30
20
10
Diagram F
Horn Distraction All Runs Fast Workers
Quiet
Noise
Quiet
120
110
100
90
80
70
60
50
40
30
20
10
Diagram G
Horn Distraction All Runs Slow Workers
Quiet
Noise
Quiet
Hypothetical Distraction Effects
Diagram H
Quiet
Noise
Quiet

FIG. 2

matization not observable in the course of one sitting. The first problems of the second sitting were always done more rapidly than the last problems of the previous sitting. If automatization is present in the first sitting it is obscured by the presence of fatigue or other unknown factors. The third consecutive sitting shows an initial reaction-time still shorter than the last problems of the second sitting. The difference between the first and second, however, is greater than the difference between the second and third, evidence that automatization is a process of gradual approach to a limiting level.

The most conspicuous point in the examination of the graphs of the tabular evidence is the initial slowing in reaction-time at the beginning of the noise-period and the beginning of the following quiet-period. The mode is thrown toward the center of the frequency curve in all cases and evidence indicates that the effect of the noise is on all reactions, not on a select few. This means that *both the noise and the quiet have constituted distractions.* But the initial slowing is more pronounced at the beginning of the noise-period than at the beginning of the following quiet-period. In all probability this is explained by the difference in the attitudes of the *S*s at the two different times. The automobile horn has certain meanings that produce stronger emotions than are found at the beginning of the last quiet-period. Apparently it is not the absolute presence of the noise that constitutes a distraction, but the fact that we have a change in the pattern-stimulus which changes the integration determining the reaction.

Introspective evidence came in great abundance. "When the quiet came on I missed the noise and wished it had not stopped." "When the horn stopped I was astonished because I had forgotten it was going," came in substance from 5 *S*s, although the horn was so loud that no other sound could have been heard. Other *S*s said that they "missed the racket," that they were bothered by the "uproarious quiet," that they could "hear the silence" and knew they were not doing as well as they expected. Often *S*s would report that they wished the noise would stop and when it did stop they wished it had not. Occasionally an *S* reported having liked the noise because it produced an exciting effect. Most of the *S*s were certain that they worked more slowly during the noise-period, and were quite sure they worked somewhat faster when the noise stopped, a conclusion which was somewhat divergent from fact.

Viewed from a somewhat different angle, the above results place an entirely new light on our method of defining a distraction. It means that the effectiveness of one pattern to disrupt another is not at all a matter of absolute intensity but rather the presence of a stimulus gradient in a given unit of time. Whether the gradient is negative or positive, that is, descending or ascending, the mental effects on integration are much the same.

A summary of these curves would show the following points.

(1) The mean gets lower with each lapse of a week, but does not drop so much between the second and third sittings as between the first and second.

(2) The mean deviation gets constantly smaller and smaller. This may be considered the result from the dropping out of uncontrolled distracting influences and attitudes.

(3) Skewness diminishes during the distraction, an indication that the distracting effect is spread over the short reactions.

(4) A pronounced retardation is found at the beginning of the quiet-period after the noise-period. Both noise and quiet are distractive.

(5) Distraction seems to have very little effect on errors. Probably the *S*s paid more attention to accuracy than to time. There are always more errors in the first half of a period, however, than in the second half.

(6) The effect of the distraction is about equal for rapid workers and for slow workers in the first problem of the noise and quiet. But the rapid workers make a very prompt recovery from the effects of the distractions, while the slow workers require nearly the entire 6 problems before the recovery is complete. Apparently the slow workers are unable to build up automatized resistance against the distraction.

VII. WRITING-PRESSURE AND BREATHING

Morgan seems to have believed that the strength of the motor processes was in some way an index to the intensity of the mental events as differentiated from speed. There is reason to believe, in our experiment, that muscular tonus, once built up at the beginning of a distraction, tends to stay. Had we continued our sittings each over as long a period as did Morgan, there is a probability that we should have secured the same results. The general contention of Morgan, however, that the increase of speed of his *S*s during noise-intervals was accomplished through the expenditure of an unusual amount of motor energy does not seem to harmonize with our writing-pressure curve.

Noting the graphs in Diagrams C, D, and E, it is quite evident that there is almost no correlation at all between the mean of the writing-pressure curve (the dotted line) and the mean of reaction-times. These are based on only 38 runs, however, and the samples are not large enough to show results free from chance variability. But if we combine all the sittings of the entire experiment, as they are illustrated in Diagram B, we have the rather surprising illustration of the statistical fact that when we add together several non-correlated pairs of measurements, as in Tables III, IV, and IX, and the measurements of 3 *S*s not included in these tables, but whose data also showed no correlation, we may get a very reliable correlation between writing-pressure and reaction-time in the larger table. Using the formula

$$r = \frac{\Sigma xy}{\sqrt{\Sigma x^2 \cdot \Sigma y^2}},$$

we find that the correlation between the values expressed in Tables II and VI is 0.55. The cropping out of this correlation from the addition of the three smaller tables together, and the evidence not tabulated, can be explained by the fact that with enough cases the negative and positive deviations from the mean tend to cancel each other and leave values which are the real and universal effects of the distraction.

Since we have here a positive correlation between writing-pressure and reaction-time, and since we are assuming that the presence of long reaction-time is an indication of lack of automatization and of the presence of attention, it would seem that in the end we must abandon any theory that continuous and rapid work under noise conditions will be accomplished at a sacrifice of muscular energy. In all probability the functioning of motor tonus in some way or other facilitates the building of new integrations, and when these integrations are well enough established to work without interference from other pattern-stimuli, the re-enforcement of the proprioceptive excitations from motor tonus is no longer needed.

The evidences of articulation in the breathing curves for our experiment resemble the descriptions given by Morgan and in all probability were as nearly identical as possible. We do not believe, however, that it is possible to produce as exact a ratio between inspiration and expiration as he seemed to give. Due to the fact that there were inhibitions of breathing frequently occurring, it was very difficult to decide whether a given portion of the curve was to be regarded as an inspiration or an expiration.

VIII. VASO-MOTOR PHENOMENON

While the various theories for the relation of the vaso-motor phenomenon to the fluctuation of attention did not concern us here to any great degree, we had thought at the beginning of the experiment that we might investigate the possibility of a connection between the strength of the phenomenon and the degree of attention, if our facts gave us a warrant for using the phrase "degree of attention."

A review of the early theories of the relation of blood circulation to attention has been given in Pillsbury's[20] work on attention. The studies and theories show the usual appearance of early work on new and complex organic principles. There seems to be a general tendency to regard the Traube-Hering wave as a simple phenomenon rather than complex. This tendency to make mental and physical processes too simple has permeated the entire history of man's investigations in the realm of biology. If the changes in mental integration we choose to call attention have anything to do with the changes of the Traube-Hering wave, it is certain that their part may be obscured by other strong forces of non-mental origin.

The early theories that the peripheral constriction of the capillaries and arteries is accompanied by a cerebral passive dilatation must be held with considerable doubt in the face of the more recent evidence that there may be active constriction in the cerebrum. Huber[21] found evidences of efferent fibres terminating in the cerebral capillaries. Wiggers[22] showed the possibility of active constriction in the cerebrum. Shepard[23] found evidences of correlation between vaso-constriction of the cranium, in trephined cases, and vaso-constriction of the periphery. We do not wish to treat of this controversy in detail, but merely to indicate that at the present time any anaemia theory of the relation of blood circulation to attention is certain to find strong opposition in some scientific quarters. We may not assume here with any great degree of safety that the adjustment of blood circulation is the cause of attention.

But there seems to be a considerable amount of evidence that the objectively controlled changes in attention may bring about a change in blood circulation. Angell and Thompson[24] reported certain characteristic shapes of the vaso-motor curve with mental events. If attention were to be considered as a state, unvarying and constant, and if there were a constant correlation between the amount of vaso-constriction and the degree of attention, we should expect to find the degree of constriction tending to take on a certain level and keeping that level as long as the attention remained constant. But anyone who has ever worked with the vaso-motor phenomenon knows that it is comparatively easy to produce a prompt constriction (the total change occurring often in as short a duration as three seconds) by all sorts of stimuli, but that it is very difficult to secure an equally prompt vaso-dilatation. The process seems to work rapidly only one way; the dilatation is usually slow when it is of the *active* (nerve-controlled) type. Passive dilatation may be secured just as rapidly as active constriction. We may note from Angell and Thompson's work that the most characteristic process is a rapid initial constriction followed by a slow dilatation for any and all stimuli except possibly the mental arithmetic, from which they did not seem to secure curves with uncontroversial evidence. But if the term attention cannot refer to a constant condition, but always becomes complicated with gradual automatization, then it is very possible that the degree of peripheral vaso-constriction is partially correlated with the degree of attention, assuming that the greatest degree of attention is secured only at the beginning of a work-period.

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[20] W. B. Pillsbury, *Attention*, 1908, 75-80.

[21] G. C. Huber, Observations on the innervation of the intracranial vessels, *J. Comp. Neur.*, 9, 1899, 1-25.

[22] Wiggers, Action of adrenalin on the cerebral vessels, *Amer. J. Physiol.*, 14, 1905, 452-465.

[23] J. F. Shepard, *Circulation and Sleep*, 1914.

[24] J. R. Angell and H. B. Thompson, A study of the relations between certain organic processes and consciousness, *Psychol. Rev.*, 6, 1899, 32-69.

The invention of the Lombard-Pillsbury plethysmograph gave the laboratory an instrument which is very delicate in recording the capillary vaso-motor changes uncomplicated with the changes of the larger arteries; although it is perfectly true that the instrument is very difficult for quantitative calibration. Our use of this instrument indicates that the same type of curves holds for initial attention as were secured by Angell and Thompson for the more violent affective and emotional stimuli. We are exceedingly skeptical concerning the possibility of any attention experiment whatever in which feeling and emotional elements are well controlled, and are inclined to believe that the conditions for both affection and attention are identical.

During the fall and winter of 1919 we had occasion to accumulate 411 records of the vaso-motor phenomenon taken with the task of giving the fluctuating responses from stimuli of ambiguous perspective and binocular rivalry. About a fifth of these records were not of much value because of the fact that some of the *S*s displayed a continuous tendency to a strong constriction due to cold hands.

There were 91 *S*s in all, 26 women and 65 men. An *S* was placed on one side of a partition, faced either with a card on which was drawn the ambiguous figure or with the stereoscope in which there was placed a card with a pair of oppositely colored squares which are ordinarily used to produce retinal rivalry. The Lombard-Pillsbury plethysmograph was fastened to the forefinger of *S*'s left hand. The right hand moved a lever laterally to and fro to indicate the changes in fluctuation, which were all indicated on the smoked-paper kymograph drum. *S* was given a normal run, then a period of from 1 to 2 min. of fluctuation responses, then a rest-period of about 2 min., then another run giving the fluctuation responses. At the beginning of the experiment *S* began with the word 'start' and stopped at the word 'rest.' Sometimes there was a terminal rest-period and sometimes there was not. We did not tabulate the vaso-motor phenomenon in the initial 'normal' period, and so we have 427 'work' periods tabulated as against 339 'rest' periods.

The following points were observed and statements of probability devised for each. (1) A rapid initial vaso-constriction followed by a slow dilatation. This meant that the constriction in the first 10 sec. must be more pronounced than at any other portion of the period. (2) A rapid initial vaso-dilatation followed by a slow constriction, or maintaining its level. (3) A level curve in which initial changes were either absent or so weak as to be unobservable. (4) An irregular curve in which the variations were deep but so constant that initial effects could not be observed. (5) Permanent constriction due to cold hands. (6) Vaso-constriction initially set up and maintained at a constant level throughout the period.

The situations above enumerated were as follows:

WORK-PERIODS
(427 sittings)
(1) 51% (2) 0.23 of 1% (3) 21.8% (4) 7% (5) 19.97% (6) 0%

REST-PERIODS
(339 sittings)
(1) 50.1% (2) 0.29 of 1% (3) 21.3% (4) 11.5% (5) 16.81% (6) 0%

The interpretation of the above shows that there is but one set of figures which warrants a positive statement; the tendency with a homogeneous series of acts for initial vaso-constriction followed by gradual dilatation. The statement is true of both rest- and work-periods.

Vaso-motor records from the distraction experiment using the motor horn as a distractor were more productive of results because of the greater reliability of the plethysmograph used. In 18% of the distraction records there was a one-to-one correspondence between problems and vaso-constriction from start to finish. In 66% of the records there were fragmentary periods of one-to-one correspondence, varying from 4 to 10 successive correspondences. The remaining records seem to show no definite correlation between the vaso-motor fluctuation and the separate problems.

In 183 distraction records there were 161 noise-periods in which there was an initial vaso-constriction stronger than in any other portion of the record. There were 94 of these records showing initial constriction in the last quiet period stronger than in any other place in that period. The measurement of vaso-motor effects during the distraction experiments merely verified the shape of the curve already attained from other evidence—that any *change* of mental processes, whether we call them attention, emotion, feeling, or what not, produces an initial constriction followed by gradual dilatation. Certainly at the beginning of the noise-period emotions were present, but this made no difference in the relative shape of the vaso-motor curve.

Although it certainly seems true that there were so many complex factors lending their forces to change the shape of the vaso-motor curve that we must be very cautious in using the Traube-Hering wave as an index to anything, it does seem that such interrelations as we have found favor a curve gradient as a concept of the nature of attention.

IX. SUMMARY

Efficiency of work performed evidently goes through a definite transition as time elapses with environmental stimuli constant. Since names should not commit one to the assumption of faculties or other mental entities, we may use some name for the nature of that mental coördination which exists at the beginning of a new and novel stimulus combination, and because this coördination seems to have certain signs which resemble the conditions described by past workers in the field of psychology as *attention* we feel justified in whatever violence we may perpetrate in using the same name for the beginning of a synthesis of reactions to a novel environmental pattern. But because the following reactions seem to display definite differences in their capacity to show efficiency in the face of foreign distracting stimuli, we wish to give another name to the behavior which follows, and again we may borrow from the field of psychology that term which suggests past evidence resembling as closely as possible the evidence in our own work. The term *habituation* has been used, but without a clear enough distinction from the word attention. The word automatization has been used in the field of abnormal psychology as indicative of behavior without consciousness or behavior without attention. Again we may not quibble concerning the justification of the use of the term *consciousness*. The behavior in the later parts of the distraction-period and the behavior of a person exhibiting automatisms shows a similarity of form and conditioning. Possibly we may say that it tends to become unconscious, and therefore unattended. From a purely behavioristic point of view we have the certain distinction that initial reactions are highly susceptible to interference from new and heretofore unexperienced stimuli. Conversely, later behavior of the same type is capable of a certain capacity to resist interference from sudden and novel stimuli. The transition from the one to the other may be called the psychodynamic gradient.

The evidence for such a concept of attention-automatization may be listed as follows.

(1) In every case the initial reaction is always longer than any other reaction of the period having constant environmental stimulus patterns. This is true of both quiet and noise periods and in excess of any training effect.

(2) There is a slight tendency for more errors to be committed in the first half of a given homogeneous period than in the second half; although there were not enough cases of error in our evidence to warrant problem-to-problem comparison.

(3) The writing-pressure and reaction-time showed a reliable positive correlation of 0.55, indicating that new integrations of behavior were accompanied by evidences of general motor tonus, and, as time passes, this tonus likewise exhibits signs of a gradient. Motor tonus becomes somewhat of an index to the degree of attention, *as we have defined attention.* At this point it is well to warn the reader that this statement of the fluctuation of motor tonus is not entirely in agreement with Morgan's statement in that he apparently holds that the tonus continues through the distraction-period and that the general average of high efficiency for that period is due, not to automatization, but to some intense mental process which he does not name.

(4) Vaso-motor concomitants likewise show initial effects which go through gradient changes as the same type of work continues under constant environmental stimuli.

(5) Fast workers, probably individuals who, either from natural aptitude or from past experience, very rapidly acquire automatization of behavior in this work, show almost an instantaneous recovery after the first problem under the noise. Slow workers acquire a facility only gradually, a fact which appears reasonable in view of the possibility that the slowness may be due to inadequate past experience or the fact that, in acts of skill, speed is the accompaniment of automatization. The recovery from the initial effect of the distraction did not fully occur until the last problem of the series. Shortness of reaction-time apparently becomes a measurement of automatization whether we are comparing different records of the same individuals or composites of records from different groups. The curve-gradient is the most conspicuous element in every picture of temporal progress.

A SIMPLE SERIES OF ABILITIES

By Edward S. Robinson, Yale University, and
Florence Richardson-Robinson

> And it should always be the aim of the experimenter not to revel in statistical methods (when he does revel and not swear) but steadily to diminish by continual improvement of his experimental methods, the necessity for their use and the influence they have on his conclusions. Statistical methods are not only ancillary; they are to the experimenter, a warning of failure. ...The experiment (if it can be called an experiment at all) is often not only bad, but its badness seems to be accepted merely as a rather interesting fact, calling perhaps for the exercise of more elaborate statistical methods—a few partial coefficients of correlation—a few rather elaborate probable errors—and that is all.—G. U. Yule

Situation in Differential Psychology

Nothing could have been of greater service to the prestige of psychology in the work-a-day world than that medley of grammar school exercises and parlor puzzles, the test of general intelligence. Although psychologists themselves have no very clear understanding of why it does what it does, a test of this type will uncover in an hour approximately as accurate information about the aptitude for study of a college freshman as his secondary school teachers could secure from a year or more of casual observation. It will tell more about the capacity of a candidate for a clerkship than will most letters from former employers. Such accomplishments should not be taken lightly. They are real; they are useful.

But technological prosperity is only one of the ends of science. As long as such prosperity rests upon a basis of rule of thumb, the conscience of science must be uneasy. Differential psychology is bristling with practical devices, but one sometimes doubts whether these devices are based upon more than a rule of thumb. On one point there must be agreement: the general theory of differential psychology has followed fairly closely in the trail of what has proved useful. The most ambitious attempt to set this domain into conceptual order, that of Spearman, has kept well within the atmosphere of practical testing. Perhaps it is well that this is so. On the other hand, it seems quite possible that the cause of theory might be benefitted if the conventional mental tests and the practical categories of intellect, skill, and character were occasionally

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neglected in favor of better defined, though more austere, procedures and conceptions. Where the issues and instruments of practicality are dominant, the ideals of experimentation are likely to receive scant homage. As Yule reminds us, the introduction of partial correlation and other elaborate statistical machinery as substitutes for experimental control ought always to be tolerated with regret.[1]

PLAN OF A SIMPLE EXPERIMENT

We shall present in this paper a study of a series of abilities which have a simplicity and a susceptibility to experimental control conspicuously and almost necessarily absent in applied psychology. We shall also offer, in the light of our results, a discussion of some of the larger problems of differential psychology.

When we come to describe our specific experimental conditions there may be doubt about the legitimacy of our conception of *ability*. There may be a question as to whether *ability* should refer to capacity or performance. Popular usage surely implies something of capacity. It is common practice in scientific work, however, to deal with abilities merely as with directly measurable performances. This will be our procedure in the present study.

We shall mean by *an ability* average performance in copying a standard line of given length under given experimental conditions. What justification have we for including within one ability a number of separate performances? What justification have we for breaking up *ability to draw lines* into *abilities to draw lines of various lengths?* Our answer is that we have no *a priori* law by means of which the single ability can be distinguished and delimited. If this is true, the essential obligation is simply that one be clear as to what, in a given investigation, is to be meant by *an ability*.

We measured the abilities of 81 *S*s in copying as accurately as possible standard lines of 10 different lengths: 6, 9, 12, 15, 18, 21, 24, 27, 30, and 33 cm. Later references to 'ability 6,' 'ability 9,' 'ability 12' will be understood to identify the abilities in terms of the various standards.

Although we know much less than *all* about the 10 abilities in question, we do know *something* about them, and that something is, in itself, quantitative and unequivocal. Furthermore, we have this quantitative and unequivocal knowledge prior to the calculation of any coefficients of correlation. It should, therefore, provide a basis for the interpretation of such coefficients when they are brought forward. The situation has striking similarity to those under which psychophysical limens have been investigated. In psychophysics there has been a constant recognition of the fact that *just noticeable differences* and the like can receive elucidation only to the degree to which there has occurred some prior and unambiguous quantification of a simpler aspect of the total situation. Indeed, our experiment is essentially within the domain of psychophysics, since the pre-psychological quantification is of a physical order. We believe, however, that chief importance lies in the more general fact that there is laid, as a basis for the understanding of such coefficients of correlation as may turn up, a clear-cut, precorrelational description of the abilities with which the experiment is concerned.

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[1] G. U. Yule, Critical notice, *Brit. J. Psychol.*, 12, 1921, 106.

The *S*s were all undergraduate women in the University of Chicago. They were tested individually by F. R.-R.

Each *S* drew 40 lines for each standard, that is to say, a total of 400 lines.

The standards were presented in irregular order. The 10 lengths were given 20 haphazard arrangements and these arrangements were set in serial order. This accounts for a total of 200 presentations. The second 200 presentations consisted in a repetition of the first. This procedure distributed the 10 lengths evenly over the total series and yet provided enough irregularity to eliminate the possibility of the *S*s anticipating a standard before its actual presentation.

The standard lines were drawn on a long strip of heavy manila paper which was mounted on double rollers in such a way that one line at a time could be made to appear at a slit in a cardboard screen. The paper was 45 cm. wide and the slit had a length of 50 cm. and a width of 4 cm. The screen was on a table and the exposure slit was at about eye-level for the seated *S*. Distance from eye to screen was 14 to 20 cm., according to the preference of the *S*. In practically all cases this distance was close to 19 cm.

The paper upon which the *S* drew was mounted on a roller under the table. From this roller it was drawn up and turned over the front edge of the table. When the *S* had drawn one of the lines she was able to push the paper strip forward in such a manner as to carry the drawing under the exposure screen and out of her sight. She was then ready for the next line which was turned into place at the exposure slit. The paper on which the drawings were made was 61 cm. in width.

The *S* was permitted to work at her chosen speed. The average time required to draw the 400 lines was about 45 min. with extremes at 30 and 60 min.

Instructions. The verbatim instructions were as follows:

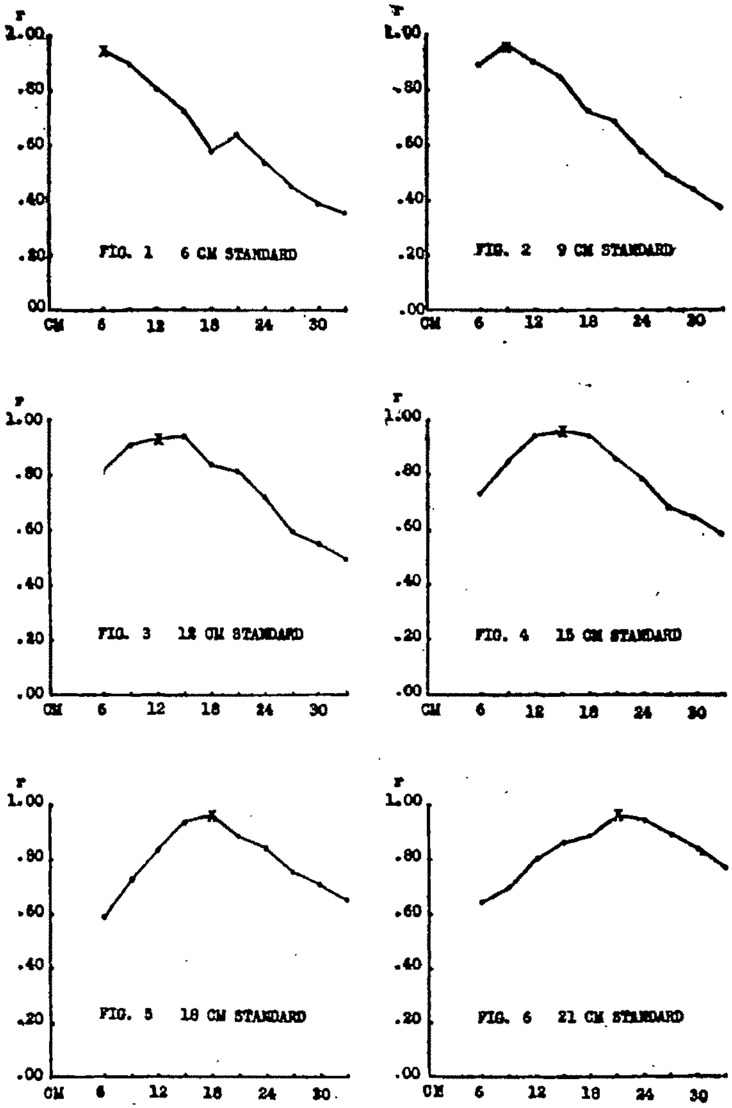
Table I (Continued)
Average Lengths Drawn by Each Subject
n for each average = 40

S	Length of Standard (mm.)									
	60	90	120	150	180	210	240	270	300	330
51	75	107	145	171	197	235	271	293	328	354
52	81	125	154	183	200	230	252	278	300	319
53	90	124	151	184	207	245	273	302	326	355
54	68	94	125	157	187	224	264	303	332	363
55	69	97	128	159	178	207	226	250	269	310
56	79	113	142	176	206	249	276	295	330	359
57	68	97	124	151	176	221	248	282	292	314
58	72	104	136	165	192	228	258	285	315	344
59	66	104	129	159	183	218	243	275	301	316
60	64	101	135	168	195	218	248	274	301	336
61	69	101	137	171	197	231	268	296	323	354
62	86	122	166	194	221	247	278	296	329	355
63	66	100	138	169	188	223	263	301	330	352
64	67	95	129	162	184	215	249	281	306	335
65	73	104	135	162	186	223	260	285	319	333
66	93	130	169	201	223	275	299	334	360	388
67	80	111	145	177	208	239	267	295	323	353
68	90	122	154	188	218	260	285	314	343	364
69	66	101	136	159	181	218	246	274	299	325
70	67	107	142	181	214	248	284	312	338	365
71	72	101	133	162	188	236	265	299	326	350
72	75	105	130	156	174	200	222	246	280	310
73	66	99	124	156	184	224	252	288	324	354
74	71	114	146	175	202	234	262	291	317	343
75	70	104	138	171	201	232	264	291	320	351
76	89	119	153	181	210	238	274	308	332	364
77	66	95	123	155	183	224	252	287	314	336
78	78	111	137	172	200	229	262	292	319	352
79	76	111	138	174	195	221	263	307	338	373
80	80	111	140	174	206	240	271	301	330	359
81	70	106	138	171	205	237	274	299	333	363

Table II
Values of *r* Among the Various Abilities

(These product-moment coefficients were calculated under the direction of Professor Clark Hull at the National Correlation Bureau, Madison, Wisconsin)

Ability	6	9	12	15	18	21	24	27	30	33
6	.9558	.8975	.8024	.7225	.5866	.6391	.5279	.4551	.3968	.3567
9	.8975	.9615	.9034	.8534	.7225	.6954	.5735	.4913	.4403	.3831
12	.8024	.9034	.9322	.9389	.8325	.8092	.7181	.5998	.5641	.4985
15	.7225	.8534	.9389	.9521	.9317	.8576	.7897	.6844	.6425	.5929
18	.5866	.7225	.8325	.9317	.9622	.8861	.8430	.7479	.7179	.6646
21	.6391	.6954	.8092	.8576	.8861	.9677	.9472	.8848	.8325	.7752
24	.5279	.5735	.7181	.7897	.8430	.9472	.9691	.9556	.9213	.8731
27	.4551	.4913	.5998	.6844	.7479	.8848	.9556	.9746	.9554	.9236
30	.3968	.4403	.5641	.6425	.7179	.8325	.9213	.9554	.9803	.9652
33	.3567	.3831	.4985	.5929	.6646	.7752	.8731	.9236	.9652	.9647



FIGS. 1–6. Each figure represents the varying values of r between a given ability and all of the other abilities. The cross marks the self-correlation and thus indicates the ability with which the others are correlated. The several abilities are represented by divisions of the base line and values of r by the divisions of the vertical axis.

used in evaluating or identifying the abilities in question, then the general form into which all of our facts can be brought will be a graphic figure of three dimensions. Height of the figure at any point will represent the magnitude of r and the other two axes will be cm. scales. One of the cm. axes will represent what we have called the points of correlational perspective, that is the linear magnitudes identifiable with the abilities with which the other abilities have been correlated. The other cm. axis will represent the linear magnitudes identifiable with the 10 individual abilities. These two axes happen, of course, to be alike in extent and in units. But the fact that the functions of Figs. 1–10 are not coincident shows the need for using these two identical scales in disparate dimensions. The reader can get a clear notion of the type of three-dimensional figure that will result if Figs. 1 and 10 are brought together. Proceeding into the third dimension, which represents the correlational perspectives taken from abilities involving greater and greater linear magnitudes, one would find the successive transverse sections of the solid figure to be planes bounded by the successive functions from 1 to 10. Or, to put the matter somewhat differently, the solid in question could be secured if the curves of Figs. 1–10 were cut out and placed one back of the other in the above order and the intervals then filled in.

Now such a tri-dimensional figure gives a very complete account of the intercorrelations among our 10 abilities. Tri-dimensional relationships are so difficult to keep in mind, however, that it would seem desirable to attempt some kind of a summary of the facts involved.

As one examines the curves of Figs. 1–10 there are seen to be certain similarities running through them. In the first place, the general slope is not greatly different for the different curves. In the second place there is evidence of a considerable stretch of what is practical linearity among those curves which take in the greater distances from the point of maximum correlation. In the third place, there seems to be a slight acceleration of fall a short distance beyond the point of maximum correlation and a slight deceleration at the other end of the curves. The situation apparent upon inspection can better be summed up, perhaps, by the statement that the curves falling in one or the other direction from the maximum would, if sufficiently extended, tend to take the form of the curve of Fig. 1 (without its apparently accidental central inversion) and that of Fig. 10. Our general estimate can be expressed in the negative proposition that there appear little grounds for dividing these curves into discrete types.

If the functions with which we are here concerned had been distinguishable into discrete types, we should have had to remain content with the tri-dimensional organization of them which has already been suggested. Under the existing circumstances generalization through averaging seemed justified. Accordingly, we calculated the average value of all the self-correlations. This average we call the zero-step average. We then obtained the average of all those r's resulting from cases where the linear differences between the members of the pairs of abilities were 3 cm., plus or minus. This we call the one-step average. After calculation of the averages up to that at the maximum linear distance, we had a series of 10 average r's that could be plotted against linear differences represented by steps from zero to plus or minus ten. This function is displayed graphically in Fig. 11.

The curve of Fig. 11 has the characteristics that seemed, upon inspection, to run through the 10 less generalized functions. The fact that it is so smooth is, in our minds, the best evidence that it is a fair summary of the curves derived from special points of correlational perspective. If the reader has considered exactly what was done in arriving at this curve of averages he realizes that the individual curves extending only 4 or 5 steps in either direction are, therefore, unrepresented at the 6th, 7th, 8th, and 9th steps. The ten-step average was, of course, derived from a single value, occurring in only 2 of the special curves, that is, the r for abilities 6 and 33. The fact that, in spite of all this, the average function is so regular would constitute a fairly adequate defense for that average as a summary of the special functions. The unequal representation of the special functions would have been likely to distort regularity if a tendency toward a common form had not permeated the individual curves.

We may now return to our primary question: What is the influence of the magnitude of the difference between any two standard lines upon the magnitude of the intercorrelation between the abilities identifiable with those standards? The answer is that r varies from about 0.35 to about 0.96 with decreases in the length-difference within the limits studied. There is no evidence, however, that the limits of our study have taken in anything like the total range of effectiveness of this variable length-difference. As a factor determining correlation its influence is, therefore, surely and considerably greater than is represented by the variation of r within the range from 0.35 to 0.96.

Furthermore, from the fact that the curve of average values (Fig. 11) has astonishing regularity, it seems justifiable to abstract the relationship between r and length-differences from the particular differences investigated.

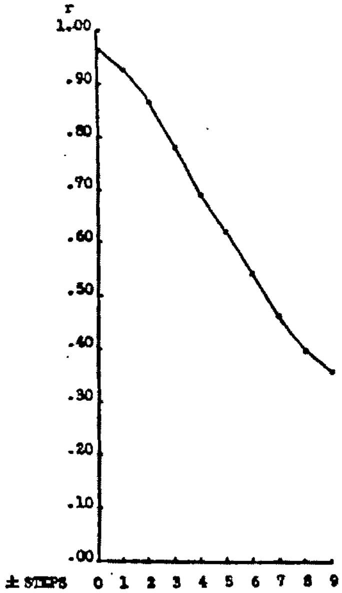


FIG. 11. Varying values of r for varying amounts of length-difference between correlated abilities. Base line represents amounts of length-difference in steps of plus or minus 3 cm. Vertical axis represents values of r.

There is little evidence that moderate extension of the lengths used would have greatly altered the influence of length-differences upon r. Naturally we should expect something interesting to happen if, on the one hand, the lengths used included very short standards, say as short as 5 mm., or very long standards which could not be drawn with a single sweep of the arm. We purposely selected lengths the drawing of which seemed to include movements similar in *type* though differing in magnitude. We are sorry now that we did not extend our range outside of the apparently homogeneous one actually investigated.

muscular activities provides a *factor* in common, but a factor which almost surely has very little to do with the magnitude of r.

A somewhat different approach to the investigation of the magnitude of a given r makes use of the technique of partial correlation. Again the explanation of the correlation between two abilities is based upon knowledge of other variables and their correlations among themselves and with the abilities in question. The simplest use of the general method requires three variables. Having them, we may apply the widely used formula:

$$r_{12.3} = \frac{r_{12} - r_{13} \cdot r_{23}}{\sqrt{1 - r_{13}^2}\sqrt{1 - r_{23}^2}}$$

If the subscripts 1, 2, and 3 stand for three mental tests, Opposites, Analogies, and Synonyms, we are able to determine what the correlation between Opposites and Analogies would be if the ability to give Synonyms were kept constant.

Knowledge of the degree to which a given r would be reduced by keeping constant some factor common to the two correlated variables is unquestionably a forward step; but it is only a step. It is a long, long way from a description of those *gradual* variations in one ability which will increase its correlation with the other to 1.00 or decrease it to 0 or even to −1.00. We believe that the achievement of such a description implies the attainment of a degree of experimental control which would render partial correlation quite unnecessary.

The ideal *factor* of correlation is a variable condition demonstrably associated with various degrees of correlation. Length-difference in our experiment is such a factor. It can be defined in unequivocal fashion and it can be produced in perfectly definite amounts. This factor was made to produce an r of from 0.35 to 0.96 depending upon its own magnitude. The likely criticisms of length-differences as a factor of correlation would be in terms of its triviality and in terms of its lack of singularity. By triviality we refer to the fact that this variable has no immediately practical bearing upon life at large. By lack of singularity we refer to the fact that there may be other variables besides length-difference which have an influence in determining the value of any of the r's to which length-difference may be importantly related. This we should readily admit. Although one may have shown experimentally that a given r can be reduced from unity to zero through the variation of some related factor, that does not mean that there are not other variables quite as effective. *Every definite variable whose relationship to a given r can be demonstrated is a factor of that r; and the only limit to the importance of any such factor is the range of values of r with which its own several values are associated.*

We are sure that this interpretation of the meaning of *factors* is at least partially applicable beyond the range where the psychophysical nature of the abilities makes easy the definition and control of basic variables. Suppose that our concern were with the r between Analogies and Opposites in the case of a group of 9-year-old boys. A probable factor in this particular r is age, and age could be controlled. So too could practice, which has often been shown to be of importance in determining the magnitude of r.[5]

Although a factor of correlation must be a variable, it may not be a continuous one. It may represent merely division into two or more discrete classes. For example, r for any two abilities might vary with the sex of the population manifesting those abilities, and in that case *sex* would be a factor of correlation. Of course, the continuous variable is the ideal one. But a qualitative distinction can, theoretically, be quite as potent a factor.

GROUPING OF ABILITIES

The drive to classify psychological abilities has been very great. The number of distinct abilities subject to definite description and experimental control is enormous. But with few exceptions, as in the studies of musical talent, the more describable abilities have received little attention by students of individual differences. Where differential psychology has kept relatively close to experiment little use has been made of that fundamental concept of differential psychology—correlation. The 'simple process' soon became of minor interest to early students of individual differences because their aims were fundamentally practical aims and they soon discovered that the simpler and more definable abilities gave poor predictions of practical performances. Thus we find much concern today with *Intellect, Muscular Skill,* and *Character,* regarding the definitions of which there is little agreement, and almost no concern with abilities that are definable though lacking, perhaps, the humanistic appeal.

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[5] See especially G. S. Gates, Individual differences as affected by practice, *Arch. Psychol.*, 8, 1922, no. 58.

The groups of abilities with which differential psychology generally deals are fundamental to that psychology. The groups—particularly *Intellect*—are considerably more important than the observable abilities that they include. In the experiment reported in this paper, we studied 10 distinguishable abilities and we approached them with little or no notion of what grouping or groupings might develop upon the application of the correlational technique. Now, even though we attach no philosophical superiority to deduction upon the one hand or to induction upon the other, it may be worth while to consider the fundamental problem of grouping of abilities from the somewhat distinct point of view represented by our results. We feel sure that such consideration will bring old conceptions into new light, if it does not bring out some new possibilities in regard to the general problem.

The problem of grouping might be phrased somewhat as follows: From the standpoint of intercorrelation what grounds are there for bringing together several otherwise distinguishable abilities and for marking them off as a class from other abilities and groups of abilities?

If, in the case of our data, variations in length-difference were, over considerable ranges, unrelated to the magnitude of r, we should have a definite basis for gathering a number of abilities into one or a few groups. The abilities within one of these groups would manifest equal intercorrelations, or at least their intercorrelations would be independent of length-differences. Grouping by neglecting length of standard would be only one conceivable type of grouping, but, if based upon such statistical considerations as we have mentioned, it would certainly be a legitimate one.

Because of its manifest failure to allow for any unifying procedure in respect to these 10 determined abilities this citerion of equal intercorrelation may seem a foolish one. By a quite plausible, though hypothetical, extension of our empirical data, however, we believe it possible to describe circumstances under which unity based upon equal intercorrelation might well be possible.

Our best idea of the probable consequences of enlarging the range of linear magnitudes involved in our actual experiment are to be had from examination of the curve of Fig. 1. (The curve of Fig. 10 cannot be so well extended in the opposite direction because of the fact that it would soon run into a zero length.) If the linear magnitudes of our standards had been of much wider range, the continuation of Curve 1 (Fig. 1) would have had either to decrease its rate of approaching an r value of 0 or else cross the zero-line. The extreme rarity of consistently negative values among r's representing even extremely divergent types of abilities would make it seem very likely that Curve 1 would ultimately show a much more marked deceleration of fall than the one actually obtained and that there would finally appear an almost horizontal section of the curve. This would mean that there were several abilities which correlated with our ability 6 to approximately the same degree. From the correlational perspective of ability 6, these other abilities would 'look alike' and might with a certain justification be classed together. Now this is, of course, only a partial fulfillment of the condition of equal intercorrelation that we originally had in mind, though we should say here that it is a perfectly defensible basis for classification. The abilities that would 'look alike' so far as their intercorrelation with ability 6 is concerned would presumably show marked differences in intercorrelation among themselves, just as do the abilities closer to 6. But, if all of the abilities studied were far enough apart along our base line, is it not conceivable that all of the possible intercorrelations would be approximately equal? In other words, if we considered only such abilities as would be represented upon any of the curves (Figs. 1–10) only after those curves became relatively horizontal, then we might have a close approach to the case of equal intercorrelation of which we have been speaking.

Fig. 12 represents the hypothetical situation. Three relationships between the magnitude of r and linear distance between the correlated abilities are given by curves a, b, and c. The points denoting self-correlation are marked by crosses. Thus, curve a shows all the r's for ability 5 and all other abilities, curve b all the r's for ability 10 and all other abilities, curve c all the r's for ability 15 and all other abilities. The lower limits of all three curves are about 0.20 and after reaching this vicinity all become practically (though not theoretically) indistinguishable. Under these circumstances abilities 5, 10, and 15 among themselves show approximately the same value of r and for that reason they can be grouped into a single class.

It is an amusing outcome of this particular type of unification that abilities, which, from the point of view of linear magnitudes, are most alike, resist inclusion in the same class; whereas abilities which, from that standpoint, are very different will go into a single class. Thus, 5 will go into a group with 10, and 15, but 6 can not be put into that class because of its high correlation with 5. This by no means excludes 6 from entering into a general group. Presumably 6, 11, and 16 would show equal intercorrelations. This suggests, of course, that the criterion of unity here discussed would allow the formation of a large number of overlapping groups. If the starting point were 5, one set of abilities would be included; if it were 6 another group would result; and these two groups would show a large overlapping.

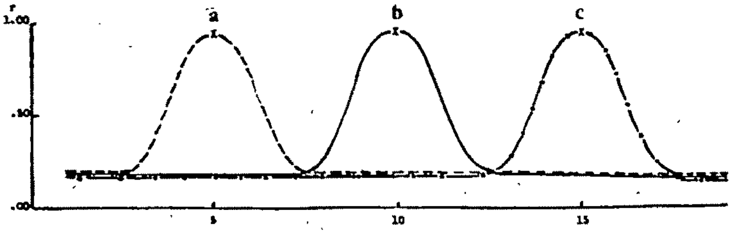

FIG. 12. An hypothetical case of relationships between linear differences and r where the range is very much greater than that in our actual experiment.

There is often an interest in determining, for a given set of r's, to what degree they are all brought about by some general common factor. Perhaps the practical coincidence of our various hypothetical curves at 0.20 represents from a new viewpoint just such a determination.

Another procedure for grouping together a number of abilities would be that of putting into the same class all of those abilities having more than some stated minimum correlation with some given ability, such as 5 in our Fig. 12. The curve, a, as it leaves the horizontal would mark one limit to the class and at its return it would mark the other limit. Or we might group around 5 only those other abilities reached before the positive accelerations in the falling curve.

As in the case of grouping on the basis of equal intercorrelation, grouping on the basis of more than minimum intercorrelation would make possible a large number of overlapping groups. The number of groups and their precise make-up would depend upon the particular abilities around which the other abilities were grouped.

The two types of grouping thus far discussed seem to us to correspond fairly closely with what is usually in mind when reference is made to *general abilities* and *special abilities.* Of course, the fact that our entire analysis rests upon the basis of a precorrelational quantification of the individual abilities makes rather hazardous any direct comparison between our analysis and those lacking in such a basis. We are inclined to believe that the usual notions of *general ability* do not include the idea of the overlapping of an indefinite number of such abilities nor the corollary that the specific constituents of one of these very general groups depends upon which one of many possible starting points (in the shape of some highly specific ability) is taken. We are also inclined to believe that the usual notions of *special ability* do not include the overlapping of such abilities which our analysis shows to be present.

The last type of grouping that we shall consider depends upon a somewhat more abstract conception than either of the first two. Inspection of the curves of Figs. 1–10 and the bringing together of those curves by averaging inclined us to accept the average curve (Fig. 11) as well representative of the relationship between r and length-difference throughout the range covered by our experiment. This would suggest, then, the possibility of throwing into one class all of those abilities to which a single law, such as that represented by Fig. 11, is applicable. If, within the limits of some single linear arrangement of abilities, orderly alterations were found in the average relationship between r and linear differences, it seems probable that those alterations would be so gradual as to make impossible any precise determination of the end of the applicability of one law and the beginning of the next. If that were true, the extent of any single class would depend upon arbitrary decision. Continuity would be outstanding; distinction would be largely a matter of convenience.

As we have gone over these various types of classification that are conceivably applicable to a series of human abilities, the persistent fact has been the arbitrariness of all of the suggested groupings. In our actual results and in our hypothetical extension of them 'natural' classes have been notably absent and continuous variation conspicuously present. Even under such circumstances there might be practical reasons why arbitrary groupings should be made. That is why we have discussed possible bases for such procedure. But when the practical need for grouping leads, as we believe it often has led, to the assumption that the needed groups must have some outstanding natural definition, there is a distortion of the probable facts.

Other Modes of Relationship Among Human Abilities

Our entire study has dealt with the interrelationships among actual and hypothetical abilities only in so far as these relations appear in values of the coefficient of correlation. Now, although the technique of statistical correlation is as important as any within the psychologist's equipment, yet it by no means exhausts the functional relationships among abilities. Transfer of learning and transfer of work decrement are two outstanding types of relationship which are not measured by the calculation of r. Such relationships may have a bearing upon r, but our understanding of the situation falls far short of permitting the prediction of one in terms of the other.[6]

A further essential characterization of our analysis is in terms of the fact that it has been, throughout, *social* in implication. We have said nothing about the relations among abilities 6, 9, and the others as they may occur in an isolated individual. It is a frequent practice in correlational studies which are really dealing with variations within some social group to couch interpretive conjectures largely in terms of the anatomical-physiological individual. We do not wish to make the negative claim that there is and can be no bridge from social correlation and differentiation to the largely non-social conceptions of anatomy and physiology (and also of general psychology), but we would insist that the bridge is still unconstructed and that the leap is full of peril.

Summary

Because students of differential psychology have had a great desire to render practical service to life at large, the investigation of logically and experimentally simple problems has been almost entirely neglected. The present paper has described an attempt to utilize the major method of differential psychology—the technique of statistical correlation—in a relatively simple and definable setting. The abilities chosen for study were such as to make reliable measurements possible. They were also open to unequivocal, quantitative description prior to the investigation of their intercorrelations, thus affording a quantitative basis for interpreting those correlations.

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[6] Some eighteen years ago Winch published a paper whose importance seems to us to have been too little recognized. In his investigation of transfer from one type of performance to another, Winch supplemented his data on transfer by obtaining intercorrelations between the performances in question, The lack of correspondence between these two equally legitimate measurements of psychological relatedness is striking and consistent. See W. H. Winch, Accuracy in school children: does improvement in numerical accuracy "transfer," *J. Educ. Psychol.*, 1, 1910, 557–589.

In light of our results we have discussed two fundamental issues of differential psychology: (1) the meaning of factors, or causes, of correlation; and (2) the nature of the grouping of simple abilities into larger classes.

We have stressed the claim that a factor of correlation is most ideally a mode of continuous variation which can be shown experimentally to be associated with rising or falling values of r. In regard to the grouping of abilities we have shown that a number of arbitrary methods are conceivable and possibly practical, but it is an outstanding feature of our own results that any sharp grouping is arbitrary rather than 'natural.' Continuity of variation from ability to ability is the rule.

THE LAW OF AFFECTIVE EQUILIBRIUM

By J. G. Beebe-Center, Harvard University

Recent experimental work has led to the formulation of many important *Gestaltgesetzen*—laws descriptive of the interdependence of parts within wholes. All of these laws, however, have dealt solely with cognitive or conative components of experience. The present paper seeks to extend the field of *Gestaltgesetzen* to include affective components of experience.[1] It describes two experiments the combined results of which show that the affective value of any one member of a sequence of experiences constituting a unitary temporal group is dependent in a definite way upon the affective values of all preceding members of the group.

I. Changes in the Affective Value of a Set of Stimuli as a Function of Repeated Presentation of a Part of This Set

Purpose. The purpose of the experiment described in the present section was to determine the difference between the affective values of a set of 21 olfactory stimuli as determined after repeated presentation of the 10 least pleasant members of that set, and as determined after repeated presentation of the 10 most pleasant members of that set.

Technique. The stimuli were 5 cc. of each of the olfactory substances listed below. These stimuli will hereafter be symbolized by the letters affixed to their names in the list.[2]

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[1] From the Harvard Psychological Laboratory. The writer wishes to acknowledge his indebtedness to Professor Edwin G. Boring for much guidance and assistance.

[2] These olfactory substances were procured from J. Méro & Boyveau, 25 Rue des Archives, Paris. Their French designations are given below to insure complete identification.

A = Essence d'Anis de France rectifiée
B = " de Portugal zeste extra
C = " " Girofle Amboine
D = Extraflor Cassie No. 30
E = Essence de Bergamote zeste extra
F = " " Cannelle Ceylan Importée
G = " " Jasmin synthet. s/fleurs
H = " " d'Ylang-Ylang Manille
I = " " de Cananga
J = Extraflor Oeillet Mignardise No. 30
K = Essence de Rose Synthet. s/fleurs
L = Essence de Genièvre baies
M = " de Petit Grain surfine
N = " " Geranium de Grasse
O = " " Fenouil doux
P = " " Neroli bigarade Algérie
R = " " Muscade noix
R = " " Thym blanche de France
S = " " Patchouly mondée
T = " " Romarin éperlé
U = " " Cumin

A = Oil of Anis
B = " " Sweet Orange
C = " " Cloves
D = Extract of Cassie
E = Oil of Bergamot
F = " " Ceylon Cinnamon
G = " " Jasmin (synthetic)
H = " " Ylang
I = " " Cananga
J = Extract of Carnation
K = Oil of White Rose (synthetic)

L = Oil of Juniper
M = " " Petit Grain
N = " " Geranium
O = " " Fennel
P = " " Neroli Bitter
Q = " " Nutmeg
R = " " Thyme
S = " " Patchouly
T = " " Rosemary
U = " " Cumin

The *Os*, three in number, were Messrs. J. R. Butler, H. R. De Silva, and T. Karwoski. All the *Os* were graduate students of psychology. They will be symbolized hereafter by the first two letters of their surnames.

The approximate rank order of the 21 stimuli had been determined for every *O* in the course of previous experiments involving these stimuli. On the basis of this information there were constituted, in the case of each *O*, three sets of stimuli the characteristics of which were as follows:

(*a*) *Complete Set:* this set consisted of all the 21 stimuli (this set was the same for all *Os*).

(*b*) *Partial Set U:* this set consisted of the 10 stimuli of the Complete Set which were most unpleasant to the individual *Os*.

(*c*) *Partial Set P:* this set consisted of the 10 stimuli of the Complete Set which were most pleasant to the *Os*.

Fig. 1 illustrates this procedure in the case of *Ka*.

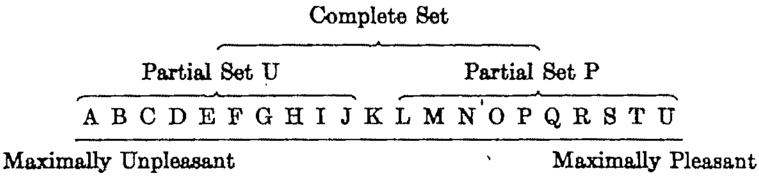

Fig. 1. $O = Ka$

The experiment consisted of four parts. Parts 1 and 3 were 'Determining Series,' *i.e.* experimental series the sole purpose of which was to determine changes in the subsequent affective judgments of the *Os*. Parts 2 and 4 were 'Test Series,' *i.e.* experimental series carried out to test any change in the affective judgments of the *Os* determined by Parts 1 and 3.

Part 1 consisted of 5 trials, each trial lasting one hour. During each trial the 10 stimuli which belonged for an *O* in Partial Set U (*i.e.* were the most unpleasant) were presented to that *O* in pairs, as in the method of paired comparisons, each stimulus being paired with every other stimulus in the set. The total number of presentations of each stimulus, during the 5 trials, was 5 x 9 = 45.

Instructions to the *Os* were as follows:

"There will be presented to you a series of successive pairs of smell stimuli. In the case of each pair, each stimulus will be presented for 5 sec. Sniff twice of each stimulus and judge which of the two experiences is the more pleasant. State your judgment in the terms: 'First more pleasant,' 'Second more pleasant,' or 'Equally pleasant.'

"In addition, please judge whether each of the experiences is absolutely pleasant, unpleasant, or indifferent. State your judgment in terms such as 'First pleasant, second unpleasant,' 'First indifferent, second pleasant,' 'Both pleasant,' etc."

As the sole purpose of Part 1 was to determine changes in the subsequent affective judgments of the *O*s, the data secured in Part 1 do not appear in the present discussion, although the judgments of the *O*s were recorded exactly as though Part 1 had been a 'Test Series.'

Part 2 consisted of 5 trials, each of one hour's duration. In the course of the 5 trials the 21 stimuli in the Complete Set were presented to the observers in pairs, each stimulus of the set being paired with every other stimulus of the set. Each stimulus was presented 4 times during each trial, and consequently 20 times in all. The instructions to the *O*s were those used in Part 1. Although these instructions called for both relative and absolute judgments (*vide supra*), only the absolute judgments were used in the computation of results. The results are stated in terms of Percentages of Pleasantness defined as follows:

a) The Percentage of Pleasantness, P, of a stimulus for a given experimental series in which it has elicited p judgments "pleasant," i judgments "indifferent," and u judgments "unpleasant" is

$$P = \frac{p + i/2}{p + i + u} \cdot 100.$$

b) The Percentage of Pleasantness of a set of stimuli for a given experimental series is the average of the Percentages of Pleasantness of the stimuli in the set for that experimental series.

Part 3 was the same as Part 1, except that the stimuli used in the case of each observer were those which belonged for that observer in Partial Set P (*i.e.* were most pleasant).

Part 4 was identical with Part 2.

In Fig. 2 below is indicated for *Ka* the chronological distribution of the different parts of this experiment. The distribution for the two other *O*s was practically the same as that for *Ka*.

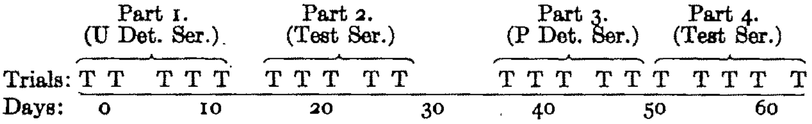

FIG. 2. $O = Ka$

Results. In Table I are given the Percentages of Pleasantness of the Complete Set as determined in Parts 2 and 4 for each *O*.

TABLE I

O	Part 2 (After Unpleasant Determination)	Part 4 (After Pleasant Determination)
Bu	82%	73%
De	33%	14%
Ka	65%	38%

Inspection of this table shows that for all *O*s the Percentage of Pleasantness of the Complete Set of 21 stimuli was greater in Part 2 of the experiment after determination with the relatively unpleasant stimuli in Partial Set U than in Part 4 of the experiment after determination with the relatively pleasant stimuli in Partial Set P.

Figs. 3–5 indicate for the three *O*s the Percentages of Pleasantness of each of the 21 stimuli of the Complete Set in Parts 2 and 4. The abscissae represent the rank order of the stimuli as determined by their respective Percentages of Pleasantness in Part 2.[3] The ordinates represent the Percentages of Pleasantness of the individual stimuli. The values of the Percentages of Pleasantness determined in Part 2 are indicated by a continuous line; those determined in Part 4, by a broken line.

Inspection of Figs. 4 and 5 shows that the change in Percentage of Pleasantness of the Complete Set of stimuli was evenly distributed over very nearly all stimuli, *i.e.* that this change did not consist of a large shift in the affective value of a few stimuli, but in a shift of the affective value of the set as a whole.

Fig. 3 shows that in the case of *Bu* the changes in the Percentage of Pleasantness of the individual stimuli from Part 2 to Part 4 were decreases in the case of 6 stimuli and increases in the case of 4. This apparent inconsistency with the results for the two other *O*s is adequately accounted for, however, by the slight difference in the case of this *O* between the Percentages of Pleasantness of the Complete Set in Parts 2 and 4, and the consequent probability that uncontrolled variability would obscure results with respect to individual stimuli.

Interpretation of results. The results stated above may be construed as consequences of either of two hypotheses.

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[3] Stimuli of which the Percentage of Pleasantness, as determined in Part 2, were the same, were ranked according to their Percentages of Pleasantness as determined in Part 4, or, failing any distinction on this score, according to alphabetical order.

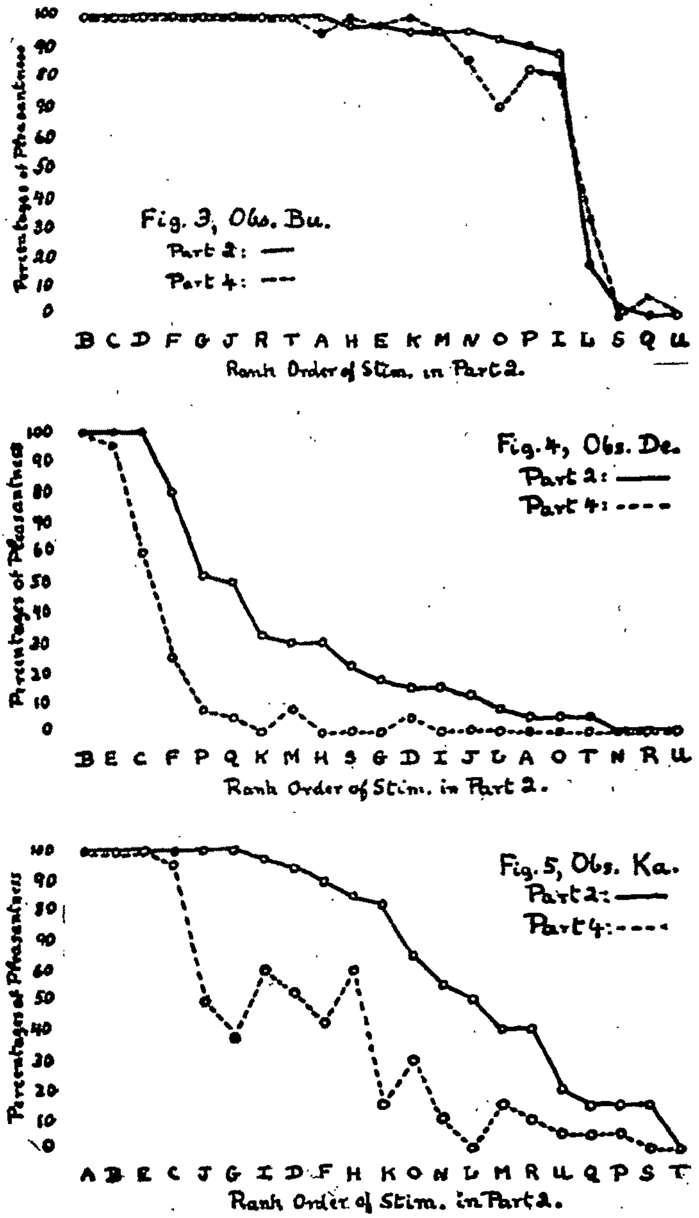
Fig. 3, Obs. Bu.
Part 2: ——
Part 4: ---
Percentages of Pleasantness
100
90
80
70
60
50
40
30
20
10
0
B C D F G J R T A H E K M N O P I L S Q U
Rank Order of Stim. in Part 2.
Fig. 4, Obs. De.
Part 2: ——
Part 4: ----
Percentages of Pleasantness
100
90
80
70
60
50
40
30
20
10
0
B E C F P Q K M H S G D I J L A O T N R U
Rank Order of Stim. in Part 2.
Fig. 5, Obs. Ka.
Part 2: ——
Part 4: ----
Percentages of Pleasantness
100
90
80
70
60
50
40
30
20
10
0
A B E C J G I D F H K O N L M R U Q P S T
Rank Order of Stim. in Part 2.

FIGS. 3–5

The first hypothesis is that the law of affective contrast, established experimentally by Bacon, Rood and Washburn,[4] holds not only in the case of successive presentations separated by brief intervals of time, but also in the case of successive presentations separated by intervals as great as 2 to 10 days. The consequences of this hypothesis in the case of an experiment similar to the one under discussion, but involving only five stimuli, are schematized in Fig. 6.

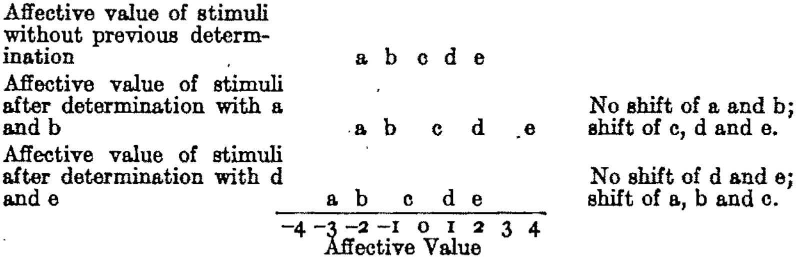
Affective value of stimuli without previous determination
a b c d e
Affective value of stimuli after determination with a and b
a b c d e
No shift of a and b; shift of c, d and e.
Affective value of stimuli after determination with d and e
a b c d e
No shift of d and e; shift of a, b and c.
−4 −3 −2 −1 0 1 2 3 4
Affective Value

FIG. 6

The second hypothesis is that the Tendency toward Dynamic Equilibrium, a principle common to Physics and Biology alike,[5] is operative in the field of affection: *the affective value of the experiential correlate of a stimulus varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group.* According to this hypothesis the difference in the affective values of the stimuli in the two Test Series would be due to the fact that in the two cases the experiential correlates of these stimuli were members of different unitary temporal groups, in one of which they were preceded by the unpleasant Determining Series (Part 1 of the experiment), and consequently tended to compensate the predominantly unpleasant affective values of the latter, whereas in the other they were preceded by the experiences of the pleasant Determining Series (Part 3 of the experiment), and consequently tended to compensate a predominance of pleasantness. The consequences of this hypothesis in the case of a simplified experiment similar to the one under discussion are indicated in Fig. 7 below.

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[4] M. M. Bacon, E. A. Rood, and M. F. Washburn, A study of affective contrast, this JOURNAL, 25, 1914, 290-293.

[5] Cf. L. J. Henderson, *The Order of Nature*, 1925, 51; also K. Lewin, *Vorsatz, Wille und Bedurfnis*, 1926, 33.

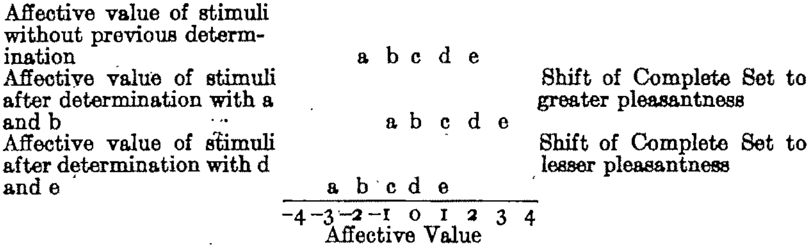
Affective value of stimuli without previous determination
a b c d e
Affective value of stimuli after determination with a and b
a b c d e
Shift of Complete Set to greater pleasantness
Affective value of stimuli after determination with d and e
a b c d e
Shift of Complete Set to lesser pleasantness
-4 -3 -2 -1 0 1 2 3 4
Affective Value

FIG. 7

To determine which of these hypotheses is correct it would be necessary to compare the affective values of the stimuli in the Complete Set, as determined in either of the Test Series, with the affective values of these stimuli as determined in a set of observations which had not been preceded by the presentation of a Determining Series. If in such a comparison it were found that changes in affective value occurred only in the case of those stimuli which had not been used in the Determining Series (cf. Fig. 6), it could be concluded that the first hypothesis (that involving an extension of the law of contrast) is correct, for this hypothesis implies a change in the affective value of a stimulus only when it follows an experience with an affective value different from its own,[6] whereas the second hypothesis is not subject to such a limitation. If, on the other hand, it turned out that there were changes in the affective value of all the stimuli, including those used in the Determining Series, it could be concluded on the same grounds as above that the second hypothesis (that involving the Tendency toward Dynamic Equilibrium) is correct (cf. Fig. 7).

The data of the present experiment do not allow such a comparison, as they yield no information on the Percentages of Pleasantness of the stimuli in the Complete Set in the absence of previous conditioning with a Determining Series. In order to secure data which would render such a comparison possible, and in order to make sure that the results of the experiment discussed above were not due to a general tendency for olfactory stimuli to become less pleasant with repetition, the writer carried out a further experiment.

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[6] Lehmann states the law of affective contrast as follows: "Das von einem gegebenen Reize erregte Gefühl ist auf die Weise von den vorhergehenden Verhältnissen abhängig, dass das Gefühl entweder schwächer wird oder in seinen Gegensatz umschlägt, wenn ein stärkeres Gefühl ähnlicher Art vorausgegangen ist." (A. Lehmann, *Die Hauptgesetze des menschlichen Gefühlslebens*, 2nd. ed., 1914, 251).

II. Distribution of Changes in Affective Value of a Set of Stimuli as a Function of Repeated Presentation of a Part of the Set

Purpose. The experiment described in the present section was performed in order to ascertain whether the change in affective value of a set of stimuli determined by repeated presentation of certain extreme members of this set is distributed evenly over all the members of the set, or whether it is confined to members other than those the repeated presentation of which determined the change.

Technique. The stimuli and general method were the same as in the experiment discussed in the preceding section. The *O*s, three in number, were Messrs. R. A. McFarland (*Mc*), J. Volkmann (*Vo*), and K. E. Zener (*Ze*).

The experiment consisted of three parts. Parts 1 and 3 were Test Series; Part 2 was a Determining Series, *i.e.* an experimental series carried out for the sole purpose of determining changes in the affective judgments of the *O*s.

In Part 1 the Percentages of Pleasantness of the 21 stimuli in the Complete Set were determined for each *O* in a series of observations involving 5 trials distributed over 11 to 12 days. Each trial lasted one hour. Each stimulus was presented 4 times during each trial, and consequently 20 times in all.

In Part 2 there were presented to each *O* in 5 one-hour trials the 10 stimuli which had proved most unpleasant to him in Part 1. Each stimulus was presented 9 times in each trial, and consequently 45 times in all.

In Part 3 the procedure was exactly the same as it had been in Part 1.

The time relations of Parts 1, 2, and 3 are indicated for *Mc* in Fig. 8. The time relations in the case of *Ze* were practically the same. *Vo* was prevented by illness from participating in the full number of trials. In his case the number of trials in each of Parts 1, 2 and 3 was reduced from 5 to 3, and the intervals between these Parts were increased to 18 days between Parts 1 and 2, and 4 days between Parts 2 and 3.

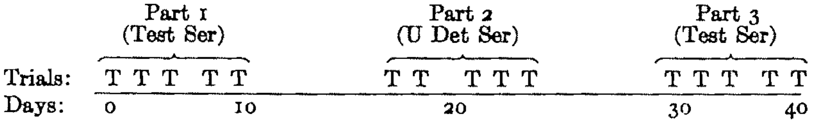

Fig. 8. $O = Mc$

Interpretation of results. It was stated at the close of the preceding section that changes in the total affective value of a set of stimuli, as a function of previous determination with certain extreme members of this set, could be construed as consequences of either of two hypotheses. The first of these hypotheses was that the law of affective contrast holds not only in the case of successive presentations separated by brief intervals of time, but also in the case of successive presentations separated by intervals as great as 2 to 10 days. The second hypothesis was that the affective value of the experiential concomitant of a stimulus varies conversely with the sum of the affective values of those experiences preceding it which constitute with it a unitary temporal group.

It was further pointed out that the implications of these two hypotheses were different with respect to the distribution of changes in the total affective value of a set of stimuli over the various members of the set. The first hypothesis implies, namely, that these changes involve only those members of the set which have not been used for purposes of determination, whereas the second hypothesis implies that these changes involve all of the members of the set.

The experiment discussed in the present section has shown that the changes in total affective value of a set of stimuli under the conditions mentioned above involve all of the stimuli in the set, including those used as determining stimuli. These results infirm the first hypothesis and confirm the second. It is concluded, therefore, that affection conforms to the following *Gestaltgesetz*, termed by the writer the law of affective equilibrium: *the affective value of the experiential correlate of a stimulus varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group.*

III. DISCUSSION OF THE LAW OF AFFECTIVE EQUILIBRIUM

Facts which can be subsumed under the law. A number of hitherto unrelated facts can, I believe, be considered as consequences of the law of affective equilibrium. A first such fact is the relativity of so-called 'absolute' affective judgments. So obvious is this relativity that Titchener uses, for the method involving such judgments, the name Serial Method, and warns the reader that "if the method is to be valid, *O* must keep in mind the serial nature of the impressions . . . the affective curve in this case is no more absolute than are the curves of the preceding experiment (by the Method of Paired Comparisons)."[9] Not only does the law of affective equilibrium imply such a relativity; it further implies the particular form which this relativity will take.

A second fact which can be considered as a consequence of the law under discussion is that almost all sensory modalities—indeed the only possible exception seems to be pain—involve both pleasant and unpleasant qualities.[10] This fact, so familiar as usually to escape mention, clearly follows from the law of affective equilibrium in view of the introspective unitariness of modalities.[11]

A third fact which can be subsumed under the law of affective equilibrium is the circumstance that experiments with absolute affective judgments never seem to involve pleasantness alone or unpleasantness alone, but both these qualities in comparable proportions.[12] G. B. Phelan, for instance, in an experiment involving a very large number of stimuli belonging to several modalities, found that, of the total number of feelings reported, 55% were pleasant and 45% were unpleasant.[13] Phelan attributes this distribution to the selection of stimuli: "These percentages," he writes, "do not express a general rule; it is merely a situation of fact. The different frequencies evidently depend upon the level."[19] By adapting Wertheimer's conception of 'level' to the needs of the case it would be possible to consider the law of affective equilibrium as the manifestation of an 'affective level.' But would this really be an explanation? I think not. A 'set,' it seems to me, is either an *ad hoc* explanatory concept, an *X* whose entire nature consists of causing the events described by some specific law, or it is a real entity with definite spatio-temporal and qualitative characteristics. If it be the former, it is nothing but an intellectual anaesthetic. If the latter, its use is legitimate only on condition that both its spatio-temporal characteristics and its qualitative characteristics be defined at least in some degree. Merely to state that it is a physiological event strikes me as a superfluous confession of faith. As I am unable at present to imagine what would be the spatio-temporal characteristics of an 'affective level,' I feel that the use of such a 'set' as an explanation of the law of affective equilibrium would bring about merely a pseudo-solution, harmful to the future of the problem.

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[9] E. B. Titchener, *Experimental Psychology*, I, pt. ii, 190, 157.

[10] "Every *O*, except *A*, reported both P and U sensations from every sense department. *A* reported them with sensations of smell, taste, and touch. . . *A* left the University before the completion of her series, and had no opportunity to observe with visual and auditory stimuli." (J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 540).

[11] "Introspection leaves no doubt that the sensations, regarded as qualitative processes, fall into a number of separate groups. All colour sensations, for instance, go together; all tonal sensations go together." (Titchener, *Textbook of Psychology*, 1910, 55).

[12] In comparable, but not in equal proportions. J. C. Flügel has shown that when *O*s record their feelings over long periods of time there is a noticeable predominance of pleasantness over unpleasantness. (Cf. J. C. Flügel, *Brit. J. Psychol.*, 15, 1925, 327 ff.). These results are in harmony with those of a large number of other experiments. Cf. the findings of G. B. Phelan mentioned in the text above; cf. also A. E. Findley, this JOURNAL, 35, 1924, 444, and C. W. Valentine, *Brit. J. Psychol.*, 6, 1913, 190. I suggest that this preponderance of pleasantness may be ascribed to the effects of tradition and learning, which eliminate situations conducive to unpleasantness (*e.g.* extremes of temperature, lack of food, putrid odors) and bring about situations conducive to pleasantness (*e.g.* physiologically favorable temperature, regular meals, perfumes).

[13] G. B. Phelan, *Feeling Experience and its Modalities*, 1925, 112.

As to some other and more satisfactory type of explanation, none has so far suggested itself to me.

SUMMARY

The present paper describes two experiments upon changes in the affective value of a set of stimuli as a function of frequent presentation of affectively extreme members of the set. The first experiment shows that the affective value of a set of olfactory stimuli, as determined 2 or more days after frequent presentation of the 10 most unpleasant members of this set, is greater than the affective value of the set as determined 2 or more days after frequent presentation of the 10 most pleasant members of the set. The second experiment shows that the changes in affective value demonstrated in the first experiment involve the entire set of stimuli, including those members whose frequent presentation have determined these changes.

From these facts it is concluded that affection conforms to the following general law, termed the law of affective equilibrium: *the affective value of the experiential correlate of a stimulus*

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[19] Cf. Max Wertheimer, *Drei Abhandlungen zur Gestalttheorie*, 1925, 100.

varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group.

In a brief discussion of this law it is pointed out that it may be construed to imply a number of hitherto unrelated phenomena, namely the relativity of so-called 'absolute' affective judgments, the affective ambivalence of sensory modalities, the occurrence of both pleasant and unpleasant experiences in almost all experiments upon affection, and the fact of affective habituation. The discussion further involves the admission that in its present formulation the law is somewhat equivocal, but this deficiency is shown to be one which can be remedied by further experimentation. The problem of explaining this law is briefly dealt with at the close of the discussion, but left unsolved.

FITTING OF FREQUENCY FUNCTIONS TO URBAN'S LIFTED-WEIGHT RESULTS

By Godfrey H. Thomson, University of Edinburgh

I. Statement of the Tail-Difficulty
II. Various Tail-Assumptions
III. Test for the Sufficiency of the Simple Tail-Assumption
IV. Rules for the Bipartite Gaussian Tail-Assumption
V. Sample Calculation of β_1, β_2, and κ_2
VI. Probable Errors of the Frequency Constants by Pearson's Formulae
VII. Theoretical Values Calculated from the Constants of Tables III and IV

The paper by H. M. Lufkin on "The Best Fitting Frequency Function for Urban's Lifted-Weight Results,"[1] has reminded me of a manuscript that I prepared on that subject in 1914 which, after lying in the possession of Professor Titchener during the war, was in 1919 awarded the prize which Urban had offered for such an essay.[2] Urban's continued residence in Europe, and the great bulk of the MS, no doubt explain how it came to be unpublished even in part. When visiting America in 1923–24 I stayed for a few days with Professor Titchener, who then returned the MS to me. Part of the MS had meanwhile supplied material for a chapter in a book on *The Essentials of Measurement,*[3] where one or two of the curves I had obtained are illustrated by diagrams.

Lufkin's table giving the Pearsonian Curve constants for Urban's lifted-weight data may be very misleading to readers who lack the time to delve into the necessary mathematics and arithmetic, because he omits to mention a fundamental difficulty in fitting Pearsonian curves to these data, a difficulty which I have called the "tail-assumption" difficulty, arising at bottom from the fact (a) that the Pearsonian equations are not in general integrable, and (b) that the Urban lifted-weight experiments did not in the case of any S extend from a point where every answer was 'heavier' to a point where every answer was 'lighter.'[4] This tail-difficulty is not referred to by Lufkin, and it is not possible from his paper to tell what tail assumption he wittingly or unwittingly made, nor can one check his figures. I have been unable to obtain the constants which he gives on any tail-assumption that I can think of.

In most of what follows I quote direct from the Urban prize essay referred to above.

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[1] This Journal, 40, 1928, 75-82.

[2] For announcements concerning this prize see this Journal, 25, 1914, 148; 26, 1915, 620.

[3] W. Brown and G. H. Thomson, Chap. IV. Skewness and heterogeneity in psychophysical data, *op. cit.*, 1921.

[4] Cf. *ibid.*, 52, 62 f., 75, 89, 92.

It will be remembered that the 'heavier' and 'lighter' data of Urban's experiments form ogives, and that the idea of fitting Pearsonian curves involves fitting not the data themselves but the differences between them. This is explained at some length in *The Essentials of Measurement* where the answers 'heavier' or Urban's *S* I are used in illustration.[5]

I. STATEMENT OF THE TAIL-DIFFICULTY

At the very outset of the work we are met by a difficulty which will be found to dominate the whole, a difficulty caused by the fact that the tails of the frequency diagram are incompletely known. Consider for example the case of the answers 'heavier' given by *S* I. Here we know that 8 thresholds are between 84 and 88 grm., 31 between 88 and 92 grm., and so on till we have accounted for 422 out of 450 thresholds. Of the remainder we only know that 27 lie somewhere above 108 grm. and one somewhere below 84 grm. We can handle the zero moment because we know the total area, but before we can find any higher moments we must make some assumption as to the distribution of these tails. For instance suppose we wish to find the first moment or, to put the same thing in another way, suppose we wish to find the arithmetical mean of these 450 thresholds, a central value which has been recommended by Spearman[6] and later by Wirth.[7] To obtain this we must assume values for the abscissae of the points at which the tails are to be concentrated; and since this will be at best a very arbitrary proceeding it seems hopeless to base thereon complicated calculations such as those necessary in employing Pearson's formulae.

It is true that this difficulty will not arise in experiments wherein the series of stimuli is so extended that these tails are completely known, that is to say series in which *S always* answers 'heavier' at the highest weight and *never* answers 'heavier' at the lowest weight. All who have carried out such experiments, however, know how difficult it is to fulfil this condition, and indeed in one kind of experiment it is impossible, namely in experiments on an absolute threshold such as the spatial threshold. The following is an example of data of such experiments, where 150 observations were taken at each stimulus value:

Cm.	0	0.5	1	1.5	2	2.5	3	3.5	4	4.5	5
Answers "Two"	5	6	8	21	56	84	105	125	141	144	148

There are five thresholds *below zero*, and it is clearly impossible to extend the stimulus series in this direction.

It will be of course be quite wrong simply to ignore such extreme values. This is precisely on a par with setting out to find the average height of Englishmen but refusing to measure any men above 6 ft. or below 5 ft. 6 in. The arithmetic mean which we need in order to apply Pearson's methods is the centroid of the whole bell curve, not of a portion of it, and similarly the higher moments must refer to the whole bell curve. The ultimate reason for this is that Pearson's curves are not generally integrable but only between certain limits. In general they reduce to an Eulerian integral.

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[5] See p. 61 (bottom half), and diagrams on pp. 62, 90.

[6] C. Spearman, The method of 'right and wrong cases' ('constant stimuli') without Gauss's formulae, *Brit. J. Psychol.*, 2, 1908, 227.

[7] W. Wirth, *Psychophysik: Darstellung der Methoden der experimentellen Psychologie*, 1912, 122.

A tail-assumption is avoidable if the curve we choose is integrable. If we wish, as we do, to use Pearson's curves we cannot integrate the equations which give the defined area and the first moment. We turn therefore to the question of what assumption we are going to make as to the distribution of the tails.

II. VARIOUS TAIL-ASSUMPTIONS

The simplest thing to do is to assume that at 80 grm. *S* I would never, and at 112 grm. would always, have answered 'heavier,' *i.e.* to center the tails at 82 and 110 grm. respectively. This is done by way of example in *The Essentials of Measurement.*[8] The calculation is continued,[9] the values $\beta_1 = 0.251$, $\beta_2 = 3.510$, and $\kappa_2 = 0.775$ are arrived at, and the conclusion is drawn that the type is Pearson's Type IV, though within the limits of probable error of β_1, β_2 and κ_2 it might be Type G, V or VII.[10]

Not only, however, are the constants β_1, β_2 and κ_2 liable to the usual probable errors of sampling. It is also clear that the whole calculation and the type of curve found depends very largely on our tail-assumption. This can be perhaps most forcibly shown by assuming that the data, instead of stopping at 84 grm. and 108 grm., had gone on in the following, not at all unlikely, fashion.

Grm.	80	84	88	92	96	100	104	108	112	116	120
Answers 'heavier'	0	1	9	40	100	186	403	423	439	446	450

If this were so we should, on carrying out similar calculations to the above find, instead of the asymmetrical Type IV, the symmetrical Type VII, with the values found for the constants being $\beta_1 = 0.000$, $\beta_2 = 4.363$, and $\kappa_2 = 0.000$. We need under these circumstances some tail-assumption which will be (1) easy to apply, (2) probable in itself, (3) yielding to the present asymmetry of the data, and (4) capable of being defined so that arbitrary personal bias cannot enter in its application. For this purpose the writer has chosen after many trials a bipartite gaussian tail-assumption, that is one which extends the data to right and left by two gauss curves, differing if necessary at each end. A middle point of the data is chosen (that nearest to 0.5 for the frequency ratio) and one gauss curve drawn through this and the top point, another through this and the bottom point. The tails are used to extend the data. The bipartite gaussian method strikes a mean between the simple assumption which exaggerates asymmetry and the ordinary (single) gaussian which exaggerates symmetry. Bipartite gaussian tails are given for all the *S*s in Tables I and II.

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[8] Brown and Thomson, *op. cit.*, 63 ff. for *S* I 'heavier.'
[9] *Ibid.*, 87 f.
[10] The Type IV curve is drawn on p. 90.

TABLE I

THE NUMBER OF ANSWERS 'LIGHTER' AT 84–108 GRM., WITH THE CONTINUATIONS CALCULATED BY THE BIPARTITE GAUSSIAN TAIL-ASSUMPTION

S	Tail			Grams Actual Data							Tail			
	72	76	80	84	88	92	96	100	104	108	112	116	120	124
I	450	449	443	421	385	261	151	55	6	3	0			
II	450	448	441	420	388	315	202	104	43	7	1	0		
III			450	448	430	391	315	171	27	12	1	0		
IV		300	297	288	277	230	132	63	15	6	1	0		
V		300	297	288	278	210	87	53	10	6	2	0		
VI		300	299	292	259	215	147	62	20	6	1	0		
VII		300	299	294	269	222	137	80	18	12	3	1	0	

TABLE II

THE NUMBER OF ANSWERS 'HEAVIER' AT 84–108 GRM., WITH THE CONTINUATIONS CALCULATED BY THE BIPARTITE GAUSSIAN TAIL-ASSUMPTION

S	Tail			Grams Actual Data							Tail			
	72	76	80	84	88	92	96	100	104	108	112	116	120	124
I			0	1	9	40	100	186	403	423	447	450		
II		0	2	10	11	50	132	238	367	407	438	448	450	
III				0	11	32	100	212	402	431	448	450		
IV		0	2	7	8	35	107	183	265	279	294	299	300	
V		0	1	8	7	48	154	204	261	283	295	299	300	
VI			0	2	7	19	56	126	223	265	291	299	300	
VII			0	2	2	20	51	131	227	246	278	293	299	300

No doubt the best method of all would be to find a curve which passed through the top and bottom points and whose course was influenced by all the other points. This could be done in various ways but the arithmetic and work would be too tedious for a mere preliminary approximation.

When to the data thus extended by the bipartite gaussian tail assumption the necessary calculations are applied, the Pearsonian constants then found are as in Tables III and IV.

In both Tables III and IV:

(1) All calculations were carried out to seven significant figures although for economy of space only five are given in these summaries. Probably only five are reliable anyhow.

(2) The sign of the skewness is consistent with the ordinary convention that from 84 grm. towards 108 grm. is *positive*. In actual working, however, while this convention was used for the 'lighter' curves, the opposite convention was used for the 'heavier' curves, so that the actual data formed the first Hardy summation in each case. Hence the sign of μ_3 is in the former the same, in the latter not the same, as the sign of skewness.

These constants differ in every instance from those given by Lufkin, which I have not been able to obtain. The whole illustrates the fact that to the ordinary probable errors of the constants (in any case doubtful since the data

TABLE III

PEARSONIAN CONSTANTS FOR THE ANSWERS 'LIGHTER'

S	Mean	μ_2	μ_3	μ_4	β_1	β_2	κ_2	Mode	Approx. Median	Skewness	Strict Type	Types Calculated
I	93.324	2.0081	0.6109	12.730	0.04608	3.1568	+0.200	93.900	93.516	−.1016 ±.039	IV	IV,G,III, Compounds
II	95.057	2.8223	1.2724	24.208	0.07202	3.0391	−0.399	96.003	95.373	−.1406 ±.038	I	I,G, Charlier
III	97.955	1.5721	0.7827	8.5019	0.15766	3.4399	0.302	98.840	98.250	−.1762 ±.038	IV	IV,G.
IV	95.453	2.0080	0.4242	15.228	0.02222	3.7767	0.011	—	—	−.0518 ±.051	IV	VII,G.
V	94.413	1.8827	0.7527	15.427	0.08489	4.3524	0.027	—	—	+.0874 ±).088	IV	VII,G,Compounds
VI	95.346	2.2667	0.0657	14.285	0.00037	2.7804	−0.000	—	—	−.0070 ±.048	I	II,G.
VII	95.8	2.3708	0.9657	19.018	0.06998	3.3835	0.096	95.106	95.569	+.1126 ±.049	IV	IV,G.

Here the sign of μ_3 is the same as that of the Skewness.

TABLE IV

PEARSONIAN CONSTANTS FOR THE ANSWERS 'HEAVIER'

S	Mean	μ_2	μ_3	μ_4	β_1	β_2	κ_2	Mode	Approx. Median	Skewness	Strict Type	Types Calculated
I	99.697	1.5876	0.8063	9.1890	0.16248	3.6456	0.158	100.516	99.970	−.1623 ±.039	IV.	IV,G,III, Compounds
II	99.306	2.4833	0.3435	22.374	0.00771	3.6283	0.005	—	—	−.0320 ±.042	IV.	VII,G.
III	99.457	1.4327	0.6482	7.5189	0.14286	3.6629	0.124	100.171	99.965	−.1491 ±.039	IV.	IV,G.
IV	98.28	2.2351	0.0427	20.591	0.00016	4.1218	0.000	—	—	+.0038 ±).049	IV.	VII,G.
V	97.2	2.3433	1.3240	19.718	0.13623	3.5908	0.137	96.285	96.895	+.1495 ±.047	IV.	IV,G.
VI	100.826	2.1306	0.4598	15.652	0.02186	3.4479	0.020	—	—	−.0588 ±.051	IV.	VII,G.
VII	101.346	2.6200	1.7458	23.357	0.16947	3.4026	0.447	100.127	100.940	+.1884 ±.047	IV,	IV,G.

Here the sign of μ_3 is the opposite from that of the Skewness. See text.

are *difference* data) must be added the uncertainty of the tail-assumption. To clinch this I assemble here some of the various values I have obtained for β_1 for *S* I, 'heavier,' by different treatment of the tails.

Tail-Assumption	*Sheppard's correction used?*	β_1
The simplest	Yes	0.251
Symmetrical gaussian	Yes	0.083
Bipartite gaussian	Yes	0.162
Tails given above	Yes	0.000
Ignoring tails and taking N = 422	No	0.947
" " " " " " "	Yes	1.167

The last two methods are of course quite indefensible, but I tried them among others in my search for Lufkin's value 1.052.

III. Test for the Sufficiency of the Simplest Tail-Assumption

This simplest tail-assumption, as has been explained, is that at the next step in each direction all, or none, of the answers would be in a certain category. If the experiments really almost reach these points it is clear that the simple tail-assumption is justifiable. This leads to the idea of a test for the data to see whether this is so, as a guide while experimenting, if one wishes to use Pearson's curves.

It is clear that the greater the number of experiments N at each step, the nearer must we go to p = 1 or 0 before we can assume that one more step will bring us either no answers or N answers of the specified kind. We can get a rough and ready test if we assume that our middle-most stimulus is at or near p = 0.5 and if we take N = 100 and N = 1000 as typical values for N. We get then the following ranges over which the *E* must spread his (equidistant) steps if he is to use the Simple Tail-assumption.

No. of stimuli = 1 + No. of steps	Range for N = 100	Range for N = 1000
5	0.060 to 0.940	0.020 to 0.980
6	0.048 to 0.952	0.013 to 0.987
7	0.040 to 0.960	0.010 to 0.990
8	0.035 to 0.965	0.008 to 0.992
9	0.031 to 0.969	0.007 to 0.993
10	0.029 to 0.971	0.006 to 0.994

This is to say an *E* making 1000 experiments at each stimulus and using ten stimuli may use the Simple Tail-assumption only if his *S* has answered 'heavier' (or whatever answer he is considering) not more than six times at the stimulus at one end and not less than 994 times at the other end, or at any rate thereabouts.

For any other value of N an approximate range can be seen from the small table above, or the proper range can be calculated thus: Let *E* look out in Table I of Pearson's Tables the deviate corresponding to a permille of 1/N. Then let him take n − 1/n + 1 of this, where n is the number of stimuli; and for this deviate thus calculated Pearson's Table I will give the range. The reader should first calculate some of the ranges given above in this way.

Judged by this test none of Urban's curves are satisfactory at *both* ends.

IV. Rules for the Bipartite Gaussian Tail-Assumption

It must of course be emphasised that the use of a bipartite gaussian curve to give the tails is only a preliminary step; the Pearson calculation is then carried out on the data thus extended.

Let the data[11] be

	Grams	p
A	84	0.0022
	88	0.0200
	92	0.0889
	96	0.2222
B	100	0.4133
	104	0.8956
C	108	0.9400

In Table I of Pearson's Tables or in some other Table of the Probability Integral find the deviates which correspond to the permilles p at the end points A and C and at the point B where p is nearest to 0.5 (for cases where two points are exactly or nearly equidistant from 0.5 see later). We thus get:

	Grams	p	Deviate	
A	84	0.0022	−2.8521	Difference 2.6330
	88	0.0200		
	92	0.0889		
	96	0.2222		
B	100	0.4133	−0.2191	Difference (algebraic) 1.7739
	104	0.8956		
C	108	0.9400	+1.5548	

The deviate-difference from A to B is 2.6330, and, as this is a distance of four steps, the gauss curve here is proceeding in steps of 2.6330 ÷ 4, that is 0.65825, in terms of the standard deviation. Therefore the deviate for 80 grams will be 2.8521 + 0.65825, that is 3.51035. The permille corresponding to this is less than 0.001, and the number of answers *heavier* therefore less than 0.450, or *zero* to the nearest integer. Thus the extension to the left of the first line of Table II, *supra*, is explained.

Between B and C are two steps, and the deviate-difference is 1.7739. Each step is therefore 0.88695. At 112 grm. the deviate is therefore 1.5548 + 0.88695, that is 2.44175, to which corresponds a permille of 0.9927, and the number of answers 'heavier' is 450 times this, or 447. At 116 grm. the deviate is 3.3287 which corresponds to a permille greater than 0.999 and therefore to 450 answers *heavier* to the nearest unit.

If there be two points B and B^1 nearly equidistant from 0.5 then one gauss integral curve is drawn through A and B ($B<0.5$) and another through B^1 and C ($B^1>0.5$).

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[11] These are again the data of *S* I 'heavier,' here given in decimal form. In most calculations, however, the actual numbers of answers were employed.

V. SAMPLE CALCULATION OF β_1, β_2, AND κ_2

Subject I *heavier.*

Grams		Tail-Assumption	—		Bipartite Gaussian
		Continued sums (Hardy's method of finding moments)			
84		1	1	1	1
88		9	10	11	12
92		40	50	61	73
96		100	150	211	284
100		186	336	547	831
104		403	739	1286	2117
108		423	1162	2448	4565
112		447	1609	4057	8622
450 trials	50	1609	4057	8622	16505
$=9\times 50$	9	32.18	81.14	172.44	330.1
		3.575 d	9.015 S_3	19.16 S_4	36.67 S_5

Mean $= 114 - 4d = 99.697$

$3+2d = 10.151$

$\log d = 0.5533435$

$\log 1+d = \underline{0.6604439}$

1.2137874

$\log 2+d = \underline{0.7462881}$

1.9600755

$\log 3+d = \underline{0.8179324}$

$\underline{2.7780079}$

$d(1+d) = 16.36015$

$d(1+d)\,(2+d) = 91.21694$

$d(1+d)\,(2+d)\,(3+d) = 599.8019$

$2S_3 = 18.03111$

sub. $d\,(1+d) = \underline{16.36015}$

$v_2 = 1.67096$

sub. Shepp. $= \underline{0.08333}$

$\mu_2 = 1.58763$

$\log \mu_2 = 0.2007493$

$$\left.\begin{array}{ll}\log v_2 & = 0.2229661\\ \log 3 & = 0.4771213\\ \log (1+d) & = \underline{0.6604439}\end{array}\right\}\ \text{add}$$

$$\left.\begin{array}{ll}\log w & = 1.3605313\\ \log 2 & = 0.3010300\\ \log (2+d) & = \underline{0.7462881}\end{array}\right\}\ \text{add}$$

$\log y = \underline{2.4078494}$

$w = 22.93672$

$d(1+d)\,(2+d) = \underline{91.21694}$

114.15366

sub. *from* $6S_4 = \underline{114.96}$

$\mu_3 = +0.80634$

(μ_3 may be negative)

$\log \mu_3 = \bar{1}.9065182$

$\log (3+2d) = 1.0065135$

$\log 2 = \underline{0.3010300}$

$\log \mu_3 = \underline{\bar{1}.9065182}$

$\log x = 1.2140617$

$d(1+d)\,(2+d)\,(3+d) = 599.8019$

$y = 255.7699$

$x = \underline{16.37049}$

$z = 871.94229$

(Note: x is negative if μ_3 is negative)

$$\begin{array}{r} S_5 = 36.6\dot{7}\\ \underline{6}\\ 220.0\dot{6}\\ \underline{4}\\ 880.26667\end{array}$$

$\log \mu_3^2 = \bar{1}.8130364$

$\log \mu_2^3 = \underline{0.6022479}$

$\log \beta_1 = \underline{\bar{1}.2107885}$

$\beta_1 = 0.1624757$

$$
\begin{array}{lrl}
 & & \left.\begin{array}{r} 880.26667 \\ 0.83548 \\ \underline{0.02916} \end{array}\right\} \text{add} \\
\frac{1}{5}v_2 & = & \\
\text{Shepp.} & = & \\
 & & 881.13131 \\
\text{Sub. } z & = & \underline{871.94229} \\
\mu_4 & = & \underline{9.18902} \\
\log \mu_4 & = & 0.9632692 \\
\text{sub } \log \mu_2^2 & = & \underline{0.4014968} \\
\log \beta_2 & = & \underline{0.5617706} \\
\beta_2 & = & 3.645613
\end{array}
$$

In practice the calculation can be stopped at this point. From β_1 and β_2 with the aid of the diagram on p. 66 of Pearson's Tables the type of the curve can be found without actually proceeding to the calculation of the criterion κ_2. It proves to be Type IV. For explanatory purposes however we need κ_2 and calculate it as follows.

$$
\begin{array}{lrl}
\log(\beta_2+3) & = & 0.8225351 \\
\text{again} & & 0.8225351 \\
\log \beta_1 & = & \underline{\bar{1}.2107885} \\
\log \text{Numerator} & = & 0.8558587 \\
\log. \text{Denom.} & = & \underline{\bar{1}.6562732} \\
\log \kappa_2 & = & \bar{1}.1995855 \\
\kappa_2 = +\,0.158381 & &
\end{array}
$$

Sign is same as M W.

$$
\begin{array}{rrl}
4\beta_2 & = & 14.582452 \\
3\beta_1 & = & \underline{0.487427} \\
M & = & \underline{14.095025} \\
2\beta_2 & = & 7.291226 \\
3\beta_1+6 & = & \underline{6.487427} \\
W & = & \underline{0.803799} \\
\log 4 & = & 0.6020600 \\
\log M & = & 1.1490657 \\
\log W & = & \underline{\bar{1}.9051475} \\
\log \text{Denom.} & = & 1.6562732
\end{array}
$$

VI. PROBABLE ERRORS OF THE FREQUENCY CONSTANTS BY PEARSON'S FORMULAE

These probable errors have been fully worked out and tabulated by Pearson for the case of ordinary frequency diagrams. There are two complications in our case to which we must return later, namely (1) the inaccuracy introduced by the tail-assumption and (2) the fact that our data are originally in ogive form; but for the present it will be valuable to ignore these difficulties and examine our constants with the aid of Pearson's Tables. Tables V and XLIII in *Tables for Biometricians and Statisticians* give the probable error of κ_2, Tables XXXVII and XXXVIII that of β_1 and β_2, and Table XLI that of the skewness. The figure here presented shows Pearson's Diagram XLVII with the ringed point $\beta_1\beta_2$ marked for Urban's *S* I 'heavier' (with the bipartite gaussian tail-assumption), surrounded by its probability-ellipse, that is a contour of the frequency surface of the variates β_1 and β_2 for N = 450, and such that the volume enclosed is just one half the frequency. The odds are 1 in 2, or evens, that the dot representing our curve could fall (on another sample being made) outside this ellipse. Since the ellipse crosses the lines representing Types III and V, crosses the area representing VI, projects into the area representing I, and is not far from the line representing VII and the point G representing the gauss normal curve, it is perfectly clear that several equations would adequately represent the data, and that the gauss curve itself would not give a *much* more improbable fit than Type IV at the dot. The above

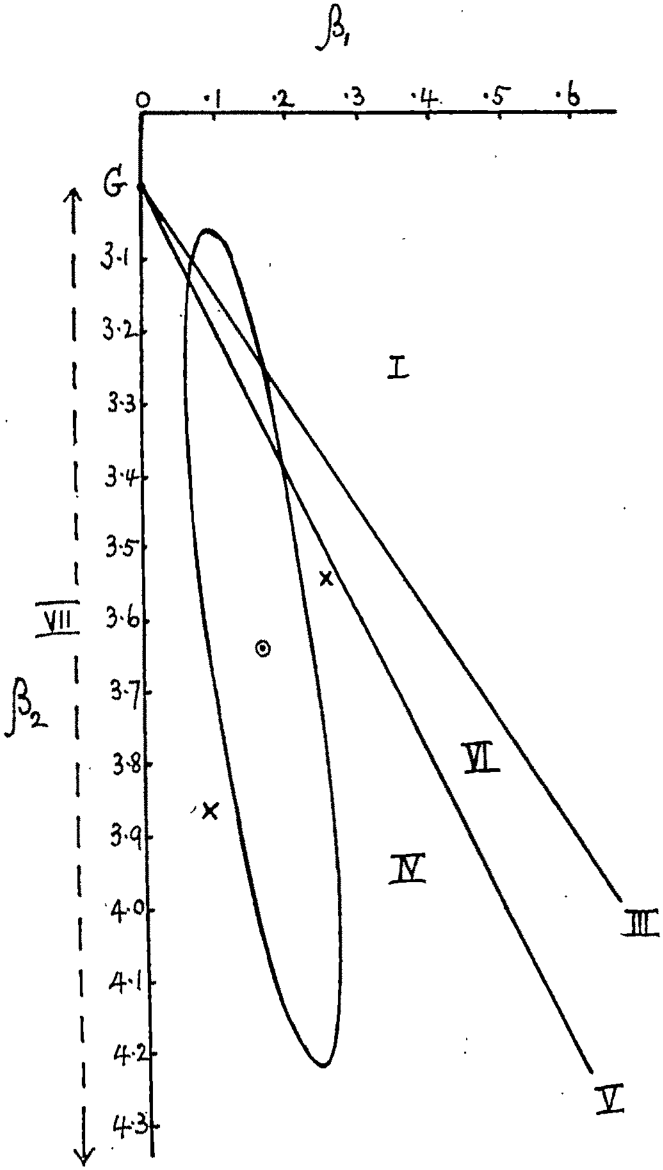

FIG. I. SHOWING PEARSON'S DIAGRAM XLVII WITH RINGED POINT SURROUNDED BY ITS PROBABILITY-ELLIPSE

probable errors, moreover, are calculated on the assumption that the tails (really assumed by a bipartite gauss curve) are actually known by observation. The fact that this is not the case introduces still further uncertainty into the determination of type. For example the simple tail-assumption, and a symmetrical gaussian tail assumption, give rise to the points marked with crosses on the diagram, each of which of course would be surrounded by its appropriate probability ellipse.

To this double uncertainty must be added yet another point connected with what may be called the Bernoulli weighting of the data. It arises from the fact that what is really observed is not a frequency diagram but points on an ogive.

The data for example of *S* I 'heavier' are in the form of proportional frequencies. Consider the answers at 100 grm. The proportional frequency of answers 'heavier' here was

$$p_{100} = \frac{186}{450} = 0.4133$$

Now if we compare this experiment at 100 grm. with drawing a ball on 450 occasions (replacing it as soon as its color is observed) from a bag containing a very large number of balls of which some are white and some black, we can write down by Bernoulli's Theorem[12] the probable error of the result p = 0.4133. This probable error will be

$$.6745 \sqrt{\frac{p\,(1-p)}{N}}$$

where N is the number (450) of trials made.[13] Our only interest here is however this, that the points of the experimental ogive are not all equally certain, although the same number of trials has been made in determining each. Each point has its own weight, and the weighting of the different parts of the frequency diagram which is derived from the experimental ogive is still more complicated.

Of this weighting the Pearsonian method takes no account.[14] It is intended for data already in the form of a frequency diagram, not for data in the form of proportional frequencies composing an ogive. Therefore (and this is our point here) the above probable errors are not the correct ones, for the complications mentioned are not consciously allowed for in any way.

We may sum up our argument of the last few pages. We can find by Pearson's Tables the probable error of the frequency constants for our data. These probable errors are so large as to cause us to doubt whether the gauss normal curve is not good enough; and there are further complications arising from the tail-assumption and the Bernoulli weights. The uncertainty of the tail-assumption will still further increase the probable error, while the effect of the unequal weighting, though small, is to increase our ignorance.

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[12] Really this is the inverse use of Bernoulli's Theorem. In any case it ought to be called rather Bayes' Theorem, and the simple formula given in the text does not hold when p is nearly zero or unity.

[13] The use of Bernoulli weights to make allowance for these probable errors was first advised by Urban, and a discussion thereon, with examples drawn from Urban's material, will be found in W. Wirth, *Psychophysik*, 1912, 148-151.

[14] By this is meant that the Pearsonian method does not consciously make allowance for those weights; but it does not, on the other hand, weight all points in the same way.

VII. Theoretical Values Calculated from the Constants of Tables III and IV

Using the given constants, the bell curves can be calculated, and from them, by a quadrature formula, the values shown in Table V, where the theoretical values are shown over a range from 80 to 120 grm., and compared with the experimental values from 84 to 108 grm.[15]

TABLE V

Comparison of Theoretical and Experimental Values

S	Grm.	Lighter	Observed	Heavier	Observed	Equal	Observed
I	80			0.0011			
	84	0.9444	−.0088	0.0045	−.0023	0.0511	.0111
	88	0.8289	.0267	0.0184	.0016	0.1527	−.0273
	92	0.6057	−.0257	0.0690	.0199	0.3253	.0058
	96	0.3262	.0094	0.2155	.0067	0.4583	−.0161
	100	0.1145	.0077	0.5006	−.0873	0.3849	.0795
	104	0.0235	−.0102	0.8078	.0878	0.1687	−.0776
	108	0.0025	.0042	0.9624	−.0224	0.0351	.0182
	112			0.9962			
	116			0.9998			
	120			0.9999			
II	72	0.9994					
	76	0.9958					
	80	0.9824		0.0017		0.0159	
	84	0.9432	−.0099	0.0092	.0130	0.0476	−.0032
	88	0.8520	.0102	0.0356	−.0112	0.1124	.0009
	92	0.6875	.0125	0.1152	−.0041	0.1973	−.0084
	96	0.4627	−.0138	0.2895	.0038	0.2478	.0100
	100	0.2379	−.0068	0.5465	−.0176	0.2156	.0244
	104	0.0858	.0098	0.7830	.0326	0.1312	−.0423
	108	0.0187	−.0031	0.9209	−.0165	0.0604	.0196
	112	0.0019		0.9762		0.0219	
	116			0.9936			
	120			0.9983			

---

[15] Other matters dealt with in the essay were, in addition to the necessary elaborations of the above outline, as follows. Examples of fitting integrable curves without a tail assumption. Discussion of the prevalence of Type IV. Tests of the probable errors by subdivision of the data. Diagrams of the strict curves, G curves, Type III curves, and in one case a Charlier curve. A discussion of the inaccuracy of Bernoulli weights at extreme values. Comparisons of accuracy of the various curves (a) based on Σv^2, (b) based on the Pearson criterion of goodness of fit. An attempt to fit compound curves. On differences in heterogeneity in the different *S*s. An attempt to make a family of ogives which could be fitted direct to the data and not to the differences. Some new tables were finally suggested. The general conclusions may be taken as summed up on p. 94 of Brown and Thomson, *op. cit.*

In 1925 the writer published certain experiments with visual stimuli which support the finding that the time of distinguishing the pleasant or unpleasant quality of such stimuli is of the order of 0.8 sec.[4] At the same time (1922, 1923), experiments of like purport were made with olfactory stimuli. The two types of stimuli had compensating advantages and limitations. The visual stimuli are relatively easy to fix and time, but their affective qualities, in ordinary laboratory settings, are slight and of complex origin. The affective qualities of the smells available, on the other hand, were relatively strong and uncomplicated, but presented difficulties, to which allusion has been made, from the point of view of reaction-timing. For the point at issue, the utmost precision in timing was of less importance than the securing of relatively uncomplicated affects.

The odoriferous substances were placed in one-ounce stoppered bottles. Five were designed to give pleasant odors, and were perfumes purchased in the market. The trade names of the perfumes were "Lily of the Valley," "Cashmere Bouquet," "Mary Garden," "La France Rose," "Blasco." Five were supposed to give unpleasant odors, these being hydrogen sulphide (solution), putrid meat, oleic acid, capryl alcohol, butyric acid. In all experiments, these stimuli were exhibited in irregular and varied order. An electric fan was used to dissipate the odors after stimulation; this was a very necessary accessory.

Under normal conditions, the only point by which the incidence of a smell stimulus can be fixed, is the beginning of a phase of inspiration when such a source of stimulation is present. These experiments deal with the time elapsing between the beginning of a phase of inspiration and the designation of the feeling evoked as pleasant or unpleasant. This measurement was attempted in two ways. In a 'kymograph' series, a pneumograph was fitted to *S*, this actuating a tambour-pen upon smoked paper, with time-line. The bottle containing the substance to be smelled was placed beneath the nose during a period of expiration. When inspiration began the substance would be smelled, and the subject said as soon as possible whether the smell was 'good' or 'bad.' These words recorded a legible disturbance of the kymograph record. Measurement was made of the time elapsing between the record of beginning inspiration and the speaking of the response word. For general operating convenience, the speed of the drum was limited to about 10 mm. per sec. It was stopped between individual observations.

In the kymograph series 11 *S*s participated, in general the same individuals who took part in the visual experiments. The total number of these observations is 126, representing in general a single run of the series with every *S*. The general average time of reporting a stimulus as pleasant or unpleasant, approximates 0.88 sec. after the beginning of inspiration. The average time of every *S*'s reactions ranges between 0.7 and 1.38 sec.; the mean variation of these quantities is 0.22 sec. There was no determinable difference between the reaction-times for the pleasant and unpleasant odors, indeed the averages for the ten odors differ little, ranging from 0.83–0.96 sec., with mean variation of 0.05 sec.

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[4] F. L. Wells, Reactions to visual stimuli in affective settings, *J. Exper. Psychol.*, 8, 1925, 64-76.

The precision of sigma units is perhaps beyond the point at issue, but the attempt was made to measure the quantities concerned in this way. These experiments are the second or 'chronoscope' series. The critical element in this technique is a device to actuate an electric contact at the beginning of an inspiratory phase. Pneumographs have been devised to do this, but were not available at the time of these experiments. The plan adopted was to suspend in an oil bath, by a fine wire, a piece of sheet copper, approximately one inch square, gold-plated. Against the face of the copper sheet rested an aluminum writing lever, actuated in the horizontal plane by a tambour connected with *S*'s pneumograph. During expiration the metal pen pressed against the suspended sheet copper, moving it through the oil bath. Upon inspiration, the pen was drawn away from the sheet copper, which was inhibited from following it by the resistance of the oil. When the aluminum pen left the sheet copper, circuit was established through the chronoscope. This was a D'Arsonval galvanometer, set up according to Klopsteg. Reaction was with the right hand if the stimulus was felt as pleasant, with the left hand if unpleasant. The movement was to raise the hand from a telegraph key, breaking the galvanometer circuit.

This set-up appeared to function satisfactorily when the inspiration was a 'sniff,' and the *S*s were instructed to make it so, but the effect of any lag not eliminated would be to shorten the reaction-time as read. Again, the 'sniff,' being sharper in this case than in the kymograph series, would induce the stimulus more promptly after stimulation began. It is perhaps due to a combination of these factors that the readings by this method are noticeably shorter than those in the kymograph series. The general average of the choice times in these observations is 555 sigma. The individual averages range between 369 and 905 sigma. The average of their mean variations is 109 sigma, relatively less than the variation in the kymograph series. Nine of the *S*s participated. The total of observations was 161. As before, no difference is disclosed between the reaction-times of the pleasant and unpleasant stimuli.

In this series were observed a few 'contrary' reactions, as where a stimulus supposed to be pleasant was reported as unpleasant. In a total of twenty observations, one *S* found capryl alcohol pleasant both times it was given, and once found "La France Rose" unpleasant. He also had three false reactions, twice to hydrogen sulphide and once to oleic acid. By the remaining *S*s one contrary reaction was recorded to hydrogen sulphide, oleic acid, and "Lily of the Valley."

Simultaneously with these experiments, series were run in which *S* was given now a bottle containing one of the odoriferous substances, now an empty bottle. The task was to react with the right hand if any smell, good or bad, was perceived, with the left if none was perceived. Eight of the *S*s took part in these experiments, a total of 159 observations. There was the curious result that to react to a smell as distinguished from no smell commonly took longer than to react to it as pleasant or unpleasant. These times range between 387 and 965, with average at 615 sigma. The m.v. of the individual averages is 145 sigma. The average reaction-time to the empty bottle is 540 sigma, the m.v. of the individual averages 109 sigma. But in 3 of the 8 *S*s the empty-bottle time was longer than the smell time, so that little weight can be attached to the difference of their averages. Two of the 8 *S*s concerned had shorter choice times to 'smell or none' than to 'pleasant or unpleasant.'

In the course of this series, there were three false reactions to the empty bottle, and one each to hydrogen sulphide and putrid meat. One *S* reported "after-images" of the smell sensations, which caused him to report smell in five of the ten cases where the empty bottle was presented to him. These were about one sixth longer than his other reactions, averaging 656 sigma, with m.v. of 93.

Techniques like Zwaardemaker's and the present, which depend for the stimulus on drawing the breath, are subject to the error of the time taken for the odor to reach the end-organ, after inspiration begins. Zwaardemaker discusses this point at some length.[5] This period of latency will vary a good deal according to the rapidity with which the breath is drawn. In his experiments it was apparently a sharp sniff, as in the present experiments of the chronoscope series. In the kymograph experiments the sniff was less pronounced, more *flairer* as he uses the term.

This may largely account for the shorter times observed in the chronoscope experiments; the substance being breathed in more sharply, reached the end-organ more quickly, and also in greater amount, causing more intense as well as more prompt stimulation. If the average of these readings, 555 sigma, be taken as representative, it bears a consistent relation to Moldenhauer's simple reaction readings, whose general average is 207 sigma. It is less consistent with Zwaardemaker's own readings (1895), which run 600–1000 sigma, or with those quoted by him from Beaunis, which average 546 sigma. The latter would be fairly consistent with the choice times recorded in the present kymograph series. But again, the shorter times quoted by Moldenhauer agree better with what is known of the simple reaction-time in general. These are some of the threads which must be disentangled before precise evaluation of data in this sphere is possible.

With reference to the main point of the inquiry,[6] however, the results seem sufficiently clear. They show correspondence in this field with the visual results, an affective choice time of less than one second, sometimes considerably less. In so far as affect may be looked on as a basic factor in emotion, it is obvious that such findings are opposed to any theory of emotions that refers them to processes having latent times longer than these. This, so far as now made out, is true of processes on which theories of the James-Lange type principally depend.

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[5] H. Zwaardemaker, *L'odorat*, 1925, 157-164.

[6] Cf. also E. A. McC. Gamble, Taste and smell, *Psychol. Bull.*, 13, 1916, 134-137; H. Henning, Psychologische Studien am Geruchsinn. *Handbuch der biologischen Arbeitsmethoden*, Abt. VI, Teil A, Heft 5, 1926, 797-802; J. H. Kenneth, Mental reactions to smell stimuli, *Psychol. Rev.*, 30, 1923, 77-79. E. Skramlik, Die Physiologie des Geruchs- und Geschmacksinnes, *Handbuch der Physiologie der niederen Sinne*, 1926, 336-339.

CONFIGURATIONAL PERCEPTION OF TACTUAL STIMULI

By B. L. ROSENBLOOM, Columbia University

The object of the following preliminary investigation[1] is to discover whether or not some of the principles found by the *Gestalt* school to govern visual perception are equally applicable to tactual perception. We propose to answer the following questions:

(1) Are some of the figures more easily perceived than others?
(2) If so, what are the characteristics of these stronger figures?
(3) Does an incomplete figure tend to be completed?
(4) Are the parts influenced by the whole figure?
(5) Does a figure have properties not contained in its parts?

Procedure. After applying the stimuli to a few skin localities, the palm of the hand was found best suited for the tactual stimulation of this experiment.

The figures used in this experiment were: a circle, which is designated in this article as (1); a square as (2); an equilateral triangle as (3); an incomplete circle as (4); three sides of a square as (5); a right angle as (6); and an incomplete triangle as (7), *i.e.* a triangle with one point missing. The area of each complete figure was 3.14 sq. in. The incomplete figures if completed also had the same area. The open circle was three-fourths complete, *i.e.* one-fourth of the circumference was missing. The two sides of the right angle were of the same length as the sides of the equilateral triangle. Of the open triangle the apex was missing. The circle, the square, and the triangle were selected because of their simplicity. The other figures employed in this experiment may be thought of as the circle, the square, and the triangle incomplete or open.

Every *S* was stimulated 5 times with each of the figures, a total of 35 stimulations. The stimulus was always applied to the same area of the palm but not always in the same position. For example, the open triangle was first presented with the open side facing the fingers, the second time the open side faced the side of the palm, the third time the open side faced the wrist, and so on. This order was maintained throughout the experiment for all *S*s. A time interval of at least 30 sec. elapsed between one stimulus and the following. Every *S* was stimulated 35 times at a single session, which lasted approximately one hour. Ten college students, 5 men and 5 women, served as the *S*s.

The figures were constructed by nailing brads through a thin wooden block four inches square. The brads were nailed closely together to form the figure desired. The *S* was stimulated by the dull ends of the brads. In order to avoid difference of pressure a one pound weight was placed on the top of each figure.

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*Accepted for publication May 5, 1928.

[1] This is a summary of a paper submitted in partial fulfillment for a Master's degree at Columbia University.

By a preliminary experiment this weight was found to be sufficient, that is to say, it was found that greater pressure does not enhance perception and that a lighter pressure is not sufficient.

During the experiment the blindfolded *S* was seated comfortably on a chair with his arms in a cast-like holder, so that he was prevented from moving his arms. The holder also supported the back of the palm when the stimulus was applied. This was done to insure constancy of stimulation and to avoid possible kinaesthesis.

The *S*s were to describe or to draw the forms perceived, and were directed to make some response even if it be a guess. They were also to indicate the degree of certainty of their responses on a scale from zero to three, *i.e.* zero representing a mere guess and three absolute certainty.

There are three criteria by which one may judge the strength of a figure, namely, the number of times a figure is reported correctly (recognized), the degree of certainty of the correct responses, and the reaction-time of the correct responses. These different standards may also be used as a check upon one another. If one figure ranks high in the number reported correct but ranks low in reaction-time, one would be unable to come to a definite conclusion as far as the relative strength of the particular figure is concerned. On the other hand, if results of one standard corroborate those of the other standards the findings are more conclusive. These criteria may therefore be employed to discover the figure value of each of the seven figures. Each criterion was used as a check upon the other two.

TABLE I

SUMMARY OF RESULTS

1	2		3		4		5
Stimulus	No. correct reports		Reaction-time		Degree of certainty		Rank based on av. ranking
	Average	Rank	Average	Rank	Average	Rank	
Circle	80	1.5	4.1	3	2.8	1.5	2.
Square	50	5.	4.9	4	2.4	5.5	3.5
Triangle	80	1.5	4.	2	2.8	1.5	1.
Op. Circle	54	4.	5.4	6	2.3	9.	6.
Op. Square	40	6.	5.3	5	2.5	3.5	3.5
Rt. Angle	74	3.	5.6	7	2.4	5.5	5.
Op. Triangle*	6	7.	3.5	1	2.5	3.5	*

*Since the degree of certainty and the reaction-time for figure 7 were based on only three judgments, it should be ignored in this summary.

Results. Table I is a brief summary of the results. Column 1 contains the figures presented as stimuli; column 2 indicates the averages of correct reports and the ranking of each figure according to this criteria; column 3 shows the average and rank based on the reaction-time of correct responses; column 4 contains the average and ranking as indicated by the degree of certainty of the correct responses; and column 5 shows the rank based on the average ranking of each figure.

From this summary it is evident that the triangle and the circle are by far the best of the seven figures. In columns 2 and 4 they are of equal ranking. Only in column 3, reaction-time, the triangle gained over the circle by one-tenth of a second. The square is the weakest of the closed figures. The three criteria give conflicting evidence regarding the figure qualities of the square; but as compared with the open figures it may be considered superior. The square and the open square are equal in figure qualities and rank next to the circle and the triangle; but if the degree of certainty were excluded in the average ranking since the average of one differs from that of the other by only one-tenth, the square would rank third undisputed. The triangle, the circle, and the square are all closed figures and it therefore appears that the complete figures are the stronger figures.

The unusual low average of the incomplete triangle in column 2 is partly due to the tendency to perceive the figure closed, *i.e.* as a triangle. The other criteria cannot be applied to this figure since they are based on only three out of the possible fifty judgments.

The open square is the most easily recognized of the open figures. The right angle ranks second among the open figures and the open circle third. If one is to judge by the number reported correctly, the only dependable criterion there is for the open triangle, that figure ranks lowest. On the whole it appears that the open figures are weaker and rank lower in figure qualities as compared with the closed figures. For a more detailed and more accurate ranking of each figure, the experimenter plans to make a more exhaustive study.

TABLE II
SUMMARY OF REPORTS

	Response						
Stimuli	Circle	Square	Triangle	Open Circle	Open Square	Right Angle	Open Triangle
Circle	40	0	0	4	0	0	0
Square	0	25	3	0	3	0	0
Triangle	0	0	40	0	0	0	1
Op. Circle	9	1	0	27	1	0	0
Op. Square	0	8	4	4	20	1	1
Rt. Angle	0	0	6	2	0	37	0
Op. Triangle	0	0	32	1	0	0	3

The next step in this experiment is to examine the records with the view to ascertain whether or not there is a tendency to perceive an open figure as closed. One need not look far to be convinced that this is the case. A large percentage of the wrong responses to all the incomplete figures were due to this tendency. Table II illustrates this tendency.

To discover whether or not a figure has properties of its own and if in turn it influences its component parts, it was decided to compare the two-point tactual threshold with the two-point tactual threshold when these points are part of a figure. If the threshold is different for the two points of an esthesiometer than for the two points when they are part of a figure, one is justified in concluding that the figure has properties of its own and that in turn it influences its component parts.

The first task was to measure the two-point threshold. The usual procedure by method of limits was followed. There were twenty ascending and twenty descending series. A time interval of about 30 sec. elapsed between successive stimulations. The same procedure was employed in obtaining the two-point threshold when the points were part of a figure. *S* was first stimulated with the movable side of the triangle wide open, giving it the appearance of three sides of a square. The open side was gradually moved closer by degrees until the subject reported the perception of a closed triangle. *E* then stimulated him with the triangle completely closed, *i.e.* with the two points of the apex close together, and gradually increased the distance between the two points until *S* reported a perception of two points of an open figure. Twenty ascending and decending series were again used. To determine the two-point threshold a compass-esthesiometer with the dull end of brads for its points were used. The location chosen for this experiment was the part of the palm nearest the figures. A triangle was constructed in the same manner as in the first part of the experiment. This triangle had one movable side so that one could adjust it to be an open triangle as figure 7 of the first part of the experiment. The movable side was so arranged that it could be opened by degrees. The brads used in the construction of this triangle were the same as those used for the points of the compass.

In this phase of the experiment the results were more conclusive than in the previous one. All *S*s reacted similarly, though in different degrees. The two-point threshold, when the points are part of a figure, is greater than the two-point threshold when the points are not part of a figure. The average two-point threshold for the compass-esthesiometer was 8.4 mm., while the average two-point threshold for the figure was 19.4 mm. This phenomenon may be explained by the tendency to perceive the open figure closed; but, whatever the explanation may be, this much seems certain: the figure has properties of its own and influences its component parts.

CONCLUSIONS

(1) The closed figures are more easily perceived than the open figures. The square seems to be the weakest of the closed figures.

(2) The circle and the triangle have the best figure qualities of the seven figures stimulated on this area.

(3) The open figures tend to be perceived as closed. In this respect the open triangle showed the strongest tendency.

(4) The figure has properties of its own and influences its component parts.

IMPROVEMENT IN MEMORY SPAN

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Memory span has usually been regarded as a congenital ability. For this reason it has been considered highly valuable as a diagnostic test of mental ability. Humpstone has shown that memory span improves rapidly and markedly with increasing age up to the college level.[1] The implication is that the limits for adult memory span are fixed at birth and beyond this fixed limit, improvement is not to be expected. In Humpstone's own words: "We may assume that there is present a definite congenital capacity. This can never be increased." But then he goes on to say that "one's capacity can be used more and more so that its effectiveness is increased."[2]

It seemed worth while to ascertain the validity of these statements. For this purpose, two highly intelligent undergraduates at the University of Pennsylvania—Mr. W. F. Kennedy (K) and Mr. R. S. Rubinow (R)—volunteered to go through a lengthy and systematic series of memory span training.

The auditory-vocal memory span for digits was used under standard conditions. The digits were read at a rate slightly faster than one per second. Every day the *S*s were given a complete series—from five digits until they broke down. Two series were given for each number of digits and the 'score' for each day was the highest single series reproduced absolutely correctly. New series were compiled and given each day.

K had 52 learning periods extending over a period of 4 months and R had 57 learning periods extending over the same period. Daily introspective reports were obtained every day from both *S*s, especially regarding the methods employed during stimulation and during recall.

The daily scores will be found in Table I for K and in Table II for R. The scores for each 10 successive days will be found in a row. The right hand columns contain the averages for these 10 trials. The improvement in the case of K is very marked—the average for the first 10 series being 8.9 and for the fifth series of 10 being 13.1—an improvement of 47%. For R the improvement was not so great; his increase being from 10.9 for the first 10 series to 14.8 for the last 10 series—an improvement of 36%. It will be noted that R started with a higher memory span than K.

These values have been plotted as curves in the accompanying Fig. Both are of the form of typical learning curves. On the other hand, the curves for

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* Accepted for publication July, 12, 1928.

[1] H. J. Humpstone, Some aspects of the memory span: A study in associability, *Univ. of Penna. Studies in Psych. and Ped.*, 8, 1917, 1-31.

[2] *Op. cit.*, 14.

the *S*s show certain marked differences in form. The curve for K begins to improve markedly after the fourteenth daily series. That for R does not improve markedly until after the twenty-fifth series.

References to the averages in the right hand columns of Tables I and II will make this difference obvious. The average improvement for K is continuous. The improvement for R is slight for the first 30 series and very rapid for the fifth series of 10-day learning periods. Both *S*s show a decrease in score at the end of the learning.

TABLE I
SHOWING DAILY SCORES FOR K

Days	Daily Scores										Ave.
1st 10	10	9	8	9	9	9	9	8	9	9	8.9
2nd 10	8	9	9	9	12	9	9	10	9	11	9.5
3d 10	10	10	11	10	11	13	12	14	12	13	11.6
4th 10	13	13	11	11	14	11	11	12	12	14	12.2
5th 10	13	13	13	11	15	11	16	13	15	11	13.1
Last 2	12	14									

TABLE II
SHOWING DAILY SCORES FOR R

Days	Daily Scores										Ave.
1st 10	12	10	9	10	11	12	12	10	11	12	10.9
2nd 10	12	10	11	10	12	12	12	11	12	11	11.3
3d 10	11	12	11	10	12	11	11	13	11	11	11.3
4th 10	11	12	13	10	13	12	11	13	11	12	11.8
5th	13	12	14	16	16	14	14	12	12	15	14.8
Last 7	15	15	13	12	13	14	14				13.7

How are these results to be interpreted? Witmer early identified memory span with the range of attention experiment when he said that memory span referred to the "number of discrete perceptions associated in a single act of attention, and the combination of the associated component parts of a single perception."[3] Since 1902, there has been a much more complete analysis of the old 'range of attention' experiment and of memory span. Both turn out to be at a higher level of mental process and to be much more complex than had been formerly supposed. The work of Oberly,[4] and Gill and Dallenbach[5] has shown that 'range of attention' is a misnomer for this experiment which, it turns out, may involve various levels of cognition and thus give various liminal values. Oberly, confirmed by Gill and Dallenbach, find normally three such levels: (1) an immediate reproduction; (2) grouping; and (3) re-imaging and counting.

Brotemarkle,[6] and Easby-Grave[7] have both shown that the mental processes underlying memory spans may be very different. They find an immediate reproductive process underlying the shorter series, for adults, while for the longer series there is grouping and organization. Easby-Grave questions whether the result of this later sort of process can be considered a true memory span.

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[3] L. Witmer, *Analytical Psychology*, 1902, 251.

[4] H. S. Oberly, The range of visual attention, cognition and apprehension, this JOURNAL, 35, 1924, 332-352.

[5] N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 256.

[6] R. A. Brotemarkle, Some memory span problems, *Psychol. Clin.*, 15, 1924, 251.

[7] C. Easby-Grave, Tests and norms at the six year old performance level, *Psychol. Clin.*, 15, 1924, 279.

More recently Oberly[8] has shown the relationship between the various levels of memory span and the same levels of 'attention' span.

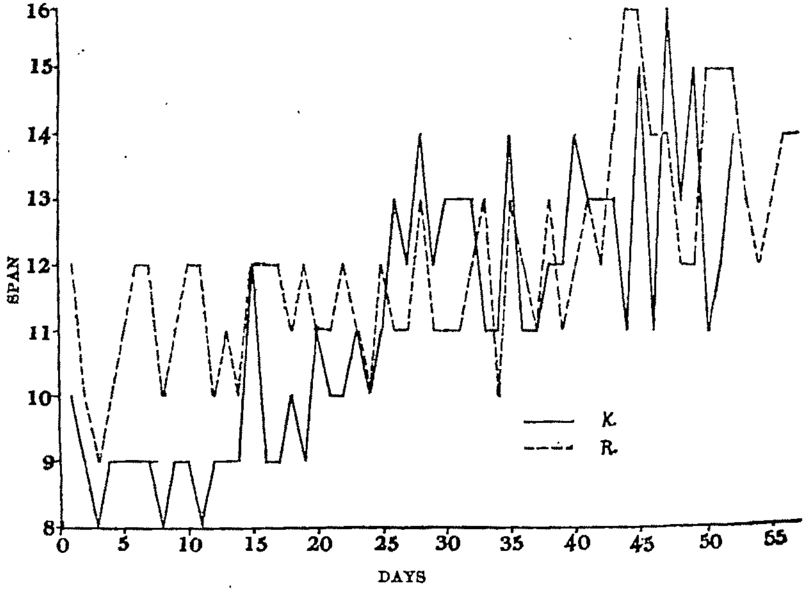

FIG. 1. Showing daily scores in memory span

Our introspective data show that the improvement all occurs at this higher level of organization. From the beginning of the training, R attempted to try various methods of grouping much more actively than K. This seems to account for R's more rapid improvement. It was only later in the experiment that K attempted this and then the improvement was marked in his case. This higher organization consisted primarily in attempting to increase the number of items in each group. This both *S*s succeeded in doing and hence materially increased their span. Eventually both became proficient in grouping by 5s. The decline for both *S*s at the end of the experiment was due, apparently, to an effort to increase the number of items in each group to six. In this they failed. Whether or not they might have succeeded had the experiment been longer continued cannot be predicted.

It is then rather obvious that no unqualified statement can be made with regard to improvement in memory span. If one means by memory span the number of items which may be immediately reproduced after a single, un-stylus had been obtained, the angles and areas of the reproductions were measured respectively with a protractor and with a planimeter. The results show no constant effect of load. If load had any effect on the reproductions, it was obscured by variables over which we had no control.

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[8] H. S. Oberly, A comparison of the spans of 'attention' and memory, this JOURNAL, 40, 1928, 295-302.

The failure of the preliminary experiments may be due to the extreme variability of the factors involved in 'cross-modal' reference.[3] Though the visual standards were constant, we have no guarantee that the *S*s had, from experiment to experiment, a constant kinaesthetic standard of reference. Indeed, it appears from our results that their kinaesthetic standard was constantly changing.

In order to avoid the complications of 'cross-modal' reference, we confined our work in the subsequent experiments to a single modality. The *S*s made a movement, and then under varying conditions of load they made a second movement, and then attempted to judge it in terms of the first.[4] Both the standard and the variable fell, therefore, within the same universe of experience.

MAIN EXPERIMENTS

Under the conditions laid down by the preliminary experiments we studied, and here report, the effect of load on judgments (1) of area, (2) of angle, and (3) of arc.

(1) *Area.* We employed the method of constant stimulus-differences, and used stimulus-areas of 5 different sizes, the middle one of which served in every case as the standard.

The stimulus-areas were square and varied in size according to the motor acuity of the different *S*s.[5] Sizes were selected that gave, in preliminary trials with an unloaded stylus, nearly 100% correct reports for the extreme comparisons. The stimulus-areas varied for our most discriminating *S*, by steps of 10, from 20 sq. cm. to 50 sq. cm.; and for our least discriminating *S*, by steps of 15, from 20 sq. cm. to 80 sq. cm.

The areas, the perimeters of which were traced blindfold by the *S*s, were demarcated by metal forms. Two right-angled metal flanges were attached to a metal base. One was fixed firmly to the base; the other was movable, and permitted rapid adjustment to any desired position. *S* used in tracing a square stylus which weighed 100 grm. The stylus was hollow, and its weight could be increased to 300 to 500 grm. by lead weights which *E* could quickly and noiselessly slip into place.

The form board was placed squarely in front of *S*. The stylus was placed within the enclosed area at the lower left hand corner. At a signal from *E*, *S* moved the stylus out along the left boundary, then across the top to the right, then down to the base, and then across to the starting point. The rate of movement was left to *S*'s discretion. When the starting point had been reached, *E* changed the size of the area, or the weight of the stylus, or both, and *S* traced again, moving in the same direction, the perimeter of the stimulus-area. As soon as he had completed the movement he reported whether the area of the second square was greater than, equal to, or less than that of the first. Doubtful judgments were permitted, but the experiments giving doubtfuls were immediately repeated.

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[3] Other experiments involving a 'cross-modal' reference have been conducted in the Cornell Laboratory. Attempts, for example, have been made to obtain an intensity of pressure equivalent to a standard sound, and contrariwise to obtain an intensity of sound equivalent to a standard pressure. The results, as yet unpublished, have shown difficulties similar to those encountered here.

[4] This method was suggested by the procedure used by H. L. Hollingworth. Cf. The inaccuracy of movement, *Arch. Psychol.*, 3, 1909, No. 13, 1-37.

[5] The *S*s of the preliminary experiments served also in this series.

The standard and variable areas were compared by all the *S*s under the following conditions. (1) The standard area was given first and a stylus of 100 grm.—*i.e.* unweighted — was used to trace both the standard and variable areas. (2) A variable area was given first and a stylus of 100 grm. was used to trace both it and the standard. (3) The standard area was traced with a stylus of 100 grm., and then a variable area was traced with a stylus of 300 grm.—*i.e.*, weighted by 200 grm. (4) A variable area was traced with a stylus of 100 grm. and then the standard area was traced with a stylus of 300 grm. (5) The standard area was traced with a stylus of 100 grm. and then a variable area was traced with a stylus of 500 grm.—*i.e.* weighted by 400 grm. (6) A variable area was traced with a stylus of 100 grm. and then the standard area was traced with a stylus of 500 grm.

During the course of a single experimental period, the standard area was compared, under every one of the 6 experimental conditions, 5 times with every one of the variables. Thus giving a daily total of 150 judgments for every *S*. The comparisons were made according to a systematic arrangement[6] which insured an equality of practice for every one of the 6 experimental conditions.

The procedure was repeated on 5 successive days. The *S*s served at the same hour every day. We thus had a grand total of 750 judgments for every *S* or 25 judgments for every *S* under every one of the 6 experimental conditions for every one of the 5 variables. The number of judgments was sufficiently large to enable us to calculate difference limens for every *S* for every one of the experimental conditions,[7] and to determine from a comparison of the limens the effect of the various loads upon the *S*s' spatial perceptions.

Results. We present the results of our study of areas in Table I, which shows the upper and lower DLs for every *S* for every one of the 6 experimental conditions.

We shall first state, in the light of the results of Weber's study with linear distances,[8] the expected effect of load on the DLs, and then we shall turn to our results to see whether the expectation was justified. If an area traversed under load appears larger than the same area traversed without load, as Weber's results indicate, then we should expect to find that our standard area, when traced with the weighted stylus, would appear, in relation to the variable area traced with the unweighted stylus, phenomenally larger, and that the effect of this would be shown in the data by an upward shift of the DLs: the upper DL would be larger and the lower DL would be smaller. Similarly, when the load was transferred to the stylus used in tracing the variable areas, the variable areas would appear, in relation to the standard, phenomenally larger, and the effect would be shown by a downward shift of the DLs: the upper DL would be smaller, and the lower DL would be larger. In brief we should expect to find that the upper DLs varied directly and the lower DLs inversely in size with the amount of the load.

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[6] E. B. Titchener, *Experimental Psychology, Instructor's Quantitative Manual,* 1905, 274.

[7] E. G. Boring, The number of observations upon which a limen may be based, this JOURNAL, 27, 1916, 315-319. L. B. Hoisington, An example of the fractionation of data from the method of constant stimuli for the two-point limen, *ibid.*, 28, 1917, 588-596.

[8] Weber, *op. cit.*

The expected results were, as Table I shows, obtained. The upper DLs show a marked tendency for all the *Ss* to become smaller as the load decreases, and the lower DLs show, conversely, a tendency though less marked to become larger. Our results corroborate Weber's findings.

Inversions from the expected result occur, however, in the data of all our *Ss*. The progression of A's lower DLs is not smooth and regular; three inversions occur in that column. It is only when the extremes are combined and compared with each other and with the mean that the relation which we have pointed out becomes evident. D's data show a regular progression until we come to the negative extreme (experimental condition 5), where the stylus load is −400 grm. At this point there is an inversion in both DLs; the upper DL becomes markedly larger whereas it should have been smaller, and the lower DL becomes smaller whereas it should have been larger. W's data show inversions in both the upper and lower DLs when the stylus load is 0.

Weber also noted inversions among the results of his *Ss*. "Reversal of error," he observed, "occurs at different intensities of load for different *Ss*."[9] They are not, he believes, accidental, but mark critical points in the experiment. He offers, however, no explanation of them; and we also are unable to account for them. D's inversions at the lowest extreme of load may be due, as Garrison has suggested in his study on the perception of distance in terms of arm-movement,[10] to a shift in attitude and in the criteria of judgment which may be engendered when extreme values in the stimulus scale are presented. As the inversions in the results of the other *Ss* occur at very different points, this explanation would suffice only for D. Other explanations must, therefore, be sought. The inversions, however, do not occur in sufficient number, nor are they sufficiently large, to invalidate the general rule that the upper DL varies directly and the lower DL inversely with the load.

The effect of load is, to a slight extent, increased in our results by a time error. The temporal order of presenting the standard and variable stimulus-areas to the *Ss* was reversed in but one of the weight conditions, that with the unweighted stylus (experimental conditions 1 and 2). The results of these two series of experiments, which may be regarded as control series, show that the upper DLs are larger when the standard is given last. The difference is small for A and D, being respectively 0.451 and 4.845, but it is quite large (9.089) for W. The effect of the time error would be to increase slightly the

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[9] Weber, *op. cit.*, 601.

[10] W. A. Garrison, The effect of varied instruction on the perception of distances in terms of arm-movement, this JOURNAL, 35, 1924, 420.

TABLE I

Results of Studies of Areas, Showing the Upper and Lower DLs in Sq. Cm., with Their Measure of Precision (h), for Every *S*, for Every One of the Six Experimental Conditions

Time Order of Standard	Weight of Standard Stylus	Experimental Condition	Subjects											
			A				D				W			
			Upper DL	(h)	Lower DL	(h)	Upper DL	(h)	Upper DL	(h)	Upper DL	(h)	Lower DL	(h)
Last	+400	6	7.405	.115	1.343	.042	23.925	.066	3.437	.046	14.080	.076	1.389	.081
Last	+200	4	6.606	.088	0.163	.083	21.524	.039	7.648	.036	14.564	.085	1.134	.075
Last	0	2	5.279	.048	3.750	.056	13.993	.051	8.125	.102	10.569	.134	13.628	.012
First	0	1	4.828	.109	0.793	.106	9.048	.049	9.953	.054	1.480	.077	13.841	.065
First	−200	3	5.917	.111	1.366	.088	4.769	.088	10.383	.037	3.375	.129	7.161	.054
First	−400	5	4.209	.077	6.076	.055	10.202	.055	7.485	.044	1.709	.071	10.631	.190

TABLE II

Results of Study of Angles, Showing the Upper and Lower DLs in Degrees, with Their Measures of Precision (h), for Every *S*, for Every One of the Six Experimental Conditions

Time Order of Standard	Weight of Standard Stylus	Experimental Conditions	Subjects															
			D				H				K				W			
			Upper DL	(h)	Lower DL	(h)	Upper DL	(h)	Lower DL	(h)	Upper DL	(h)	Lower DL	(h)	Upper DL	(h)	Lower DL	(h)
Last	+200	6	8.40	.077	6.48	.042	7.07	.141	2.11	.157	1.86	.052	8.61	.046	24.99	.066	2.04	.042
Last	+100	4	10.63	.057	7.68	.067	5.00	.067	0.94	.054	3.06	.052	10.03	.047	27.41	.034	2.71	.052
Last	0	2	7.18	.055	3.24	.038	17.03	.100	1.31	.054	0.44	.070	7.22	.059	15.01	.041	11.87	.048
First	0	1	3.27	.108	8.46	.102	4.18	.073	2.87	.071	0.53	.069	4.80	.075	3.69	.056	8.97	.111
First	−100	3	7.66	.123	7.07	.108	4.32	.113	2.56	.165	0.69	.068	5.42	.067	6.57	.118	8.48	.070
First	−200	5	8.37	.061	6.39	.058	15.95	.100	6.76	.074	2.53	.062	5.65	.060	7.94	.091	5.85	.070

TABLE III

Results of Study of Arcs, Showing the Upper and Lower DLs in Terms of x, with Their Measures of Precision (h′), for Every *S*, For Every One of the Six Experimental Conditions

Time Order of Standard	Weight of Standard Stylus	Experimental Condition	Subjects															
			D				H				K				W			
			Upper DL	(h′)	Lower DL	(h′)	Upper DL	(h′)	Lower DL	(h′)	Upper DL	(h′)	Lower DL	(h′)	Upper DL	(h′)	Lower DL	(h′)
Last	+200	6	1.53	.258	1.55	.245	0.16	.426	0.31	.411	1.49	.081	1.04	.120	2.13	.170	0.23	.222
Last	+100	4	1.27	.403	1.49	.197	0.09	.372	0.52	.404	2.75	.094	0.02	.078	0.42	.106	0.16	.205
Last	0	2	1.32	.437	1.73	.141	0.25	.795	0.58	.354	1.23	.145	0.52	.159	1.94	.378	0.42	.106
First	0	1	0.93	.256	1.09	.390	0.35	.423	0.20	.531	0.18	.381	0.47	.338	0.56	.465	0.92	.807
First	−100	3	0.53	.290	1.09	.236	0.66	.484	0.34	.427	0.03	.344	0.36	.307	0.37	.454	0.74	.360
First	−200	5	0.59	.388	0.88	.256	0.56	.428	0.20	.503	0.35	.318	1.17	.293	0.26	.443	0.75	.446

point he released the stylus and placed his hand and arm in the ready position. The procedure was then repeated with another weight of stylus, or another degree of curvature, or with a change of both, and then *S* reported whether the curvature of the second arc traced was greater than, equal to, or less than that of the first. As in the previous experiments, doubtful judgments were permitted, but the experiments giving them were immediately repeated.

The experimental variations used in the experiments with areas and with angles were used again with the arcs. The same *S*s, as in the experiment with angles, served on 5 successive days and at the same hour every day. Twenty-five judgments of the comparison of the standard and every one of the 5 variable arcs were received from every *S* under every one of the 6 experimental conditions.

Results. Upper and lower DLs and their measures of precision (h) were computed for every *S* for every experimental condition. These data are given, in Table III, in terms of x in place of D. The inverse relation between radius and curvature, and the lack of a simple relation between them, renders the transformation of the x values into D values problematical. Since we are concerned, not with the absolute size of the DL, but merely with their relative sizes, the x values serve our purpose as well as the D values.

The expected course of the DLs with arcs is the same as the DLs with areas; the upper should vary directly, and the lower inversely with the weight of the standard stylus. This prediction follows from our previous results. If an area traversed under load appears phenomenally larger, and an angle traced under load phenomenally smaller, *i.e.* more acute, then we should expect that the curvature of an arc traversed under load would appear phenomenally greater.

The expectations are realized, as Table III shows, in the results of three of our *S*s, D, K, and W. The upper DLs of these *S*s vary, with but few inversions, inversely with the load of the standard. The inversions, where they do occur, are small and appear for the most part at the extremes of the weight-series. D's data show one slight inversion, which occurs at the lowest weight extreme, −200. K's data show two inversions, both slight and both occurring at the weight extremes. W's data show one inversion, which is not large and also occurs near the upper weight extreme. The data for the lower DLs are less uniform, but the predicted result, direct variation with load, is evident in them, particularly when the extremes are compared with each other and with the mean.

H's results stand in absolute disagreement with expectation and with the results of the other *S*s. His upper DLs vary inversely and his lower DLs tend to vary directly with load. It is only fair to state, however, that H had difficulty in understanding what was meant by greater curvature. We are inclined to believe that 'greater curvature' meant for him the curve of a greater circle. His 'greater' would correspond to the 'lesser' of the other *S*s, and his 'lesser' with their 'greater.' If his results are interpretated in this light they confirm rather than negate, prediction, and corroborate rather than disagree with the results of the other *S*s.

The apparatus used in all these experiments is not as delicate as the measurement of the illusion under investigation demands. Load is not able to exert its maximum effect when metal patterns are traced. In order to overcome the frictional resistance, *S* exerts an increased effort that is greatly in excess of that demand, and consequently the demands made by the weight-conditions are partially met and, to the extent that they are met, obscured. Free movements would show the demands of load better than those restricted by forms, but the difficulty of using free movements immediately becomes apparent when an attempt is made to bring them under experimental control.

CONCLUSIONS

It is a common error to suppose that the new school of physical relativists have introduced subjectivism into physics. Curiously enough, the relativists themselves contend that they have saved the objectivity of physical phenomena.[15] There are two criteria of objectivity involved here. There is, first, the old absolutist notion that the objective is that which is independent of all observers. If this criterion is accepted, then surely Einstein is a subjectivist, for his central thesis is that the quantitative traits of space and time depend on the observers. But this criterion of objectivity is wholly useless, if not meaningless. It is difficult, even self-contradictory, to form an idea of what the universe, independent of all observers, would be like. At any rate, the question of the existence of such an independent order has been the threadbare enigma of philosophers since Protagoras, and is still the chief bone of contention between the new and the critical realists of today. There is another criterion of objectivity which leaves science free to winnow the 'real' from the 'unreal' without becoming enmeshed in metaphysical tangles. This true criterion of objectivity is that the objective is independent of any particular observer, not that it is independent of all observers. Einstein's great accomplishment was to prove that it was possible to find a common denominator between the time and space experiences of all observers. This common denominator is a law. The objective is that which is governed by law: if my experience of space rests on a general law, it is more than my private subjective experience.

The restless insurgents in psychology who espouse behaviorism often reject phenomenological data because they are 'subjective.'[16] Perhaps no two human beings exist who get the same quantitative experience in traversing a mile. Consequently the 'objective' psychologists would prefer to compel agreement by measuring the mile with a yard stick. This, of course, secures a certain amount of agreement, but not psychological agreement. To count the number of yards in a mile is only a way of avoiding a phenomenal experience. To add absurdity to absurdity, the geometer, whose task our behaviorist has usurped, should now decide that it was his business to study distances as phenomenal experiences! The 'mentalists' in psychology will take no part in this comedy because they do not hold the naïve notion that a thing, in order to be objective, must be 'really out there' independent of all observers. Whatever the variety of our kinaesthetic experience of space, that variety is still within the bounds of law. What these laws are, we have been trying to establish in this study.

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[15] Cf. A. S. Eddington, *Space, Time, and Gravitation*, 1921, Preface.

[16] Thus J. R. Kantor hails Weiss's *Theoretical Basis of Human Behavior*, as a triumph of 'objectivism' in psychology because Weiss regards psychological phenomena as ultimately 'electron-proton configurations.' Cf. *J. Phil.*, 24, 1927, 22.

From the results of these experiments the following generalizations may be drawn.

(1) A given area sensed under load appears to be phenomenally enlarged.

(2) A given angle traversed under load appears to be phenomenally more acute, *i.e.* smaller.

(3) A given arc traversed under load appears to be phenomenally more sharply curved, *i.e.* to be a curve of smaller radius.

(4) As load is increased, critical points are reached where the results due to load are reversed.

We acknowledge the tentative character of our results. They suggest, however, that the geometry of the muscles is hyperbolical, that it is the geometry of Lobachevski. We suggest that experimental study will show that visual space is also non-Euclidean. Unlike Euclid's geometry, parallel lines meet in visual space. In visual space the degree of angle and curvature of line vary with perspective. Unlike Euclid's geometry, visual space has one point which is always unique, that occupied by the observer.

INTELLIGENCE AND PIGMENTATION OF HAIR AND EYES IN ELEMENTARY SCHOOL CHILDREN

By G. H. Estabrooks, Colgate University

The following extracts deal with material which the writer collected for the Harvard Growth Study in the years 1924–1926. One of them shows a definite correlation between pigmentation and 'intelligence' which is seen to be spurious only when certain factors, very easily overlooked, are taken into account.

The pigmentation of hair and eye was secured for the entire elementary school population of New Rochelle, N. Y., in Grades VI, VII, and VIII. The work was done without the aid of a color scale and consequently is somewhat subjective; but we are concerned here only with the gross divisions of light hair-dark hair and of blue eye-brown eye, and thus we eliminate those finer distinctions wherein confusion would be most liable to arise. All the children had previously been given the Dearborn 'C' Group Test.

TABLE I

Median Intelligence Quotients
(Dearborn 'C' test).

Pigmentation Groups	Boys No.	Boys I.Q.	Girls No.	Girls I.Q.
Blue eye	275	108.9	276	109.2
Brown eye	116	105.0	126	105.5
Mixed eye*	53	——	41	——
Light hair	214	109.1	220	109.2
Dark hair	230	106.3	223	105.1
Total	444	107.7	443	107.1

*Mixed eyes are included merely to balance the numbers

In a previous article[1] the writer has pointed out why we cannot compare the intelligence quotients of different linguistic groups; but here we shall consider only the relation between pigmentation and intelligence within the North European group of these school children. Membership in this group requires both parents to be of North European extraction (stock originating in the British Islands, North Germany, or the Baltic States). Linguistic handicaps within such a school group in a city like New Rochelle are rare and probably insignificant.

Median intelligence quotients from the Dearborn 'C'-Test are given in Table I. The children are divided into gross pigmentation groups.

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*Accepted for publication May 24, 1928.

[1] G. H. Estabrooks, A proposed technique for the investigation of racial differences in intelligence, *Amer. Natur.*, 62, 1928, 76-87.

The blond child is definitely in the lead as regards 'intelligence.' We must consider, however, the fact of progressive pigmentation. A sample such as this naturally contains a wide range of ages, actually in this case running from 9–18 years. Owing to selective processes going on within the elementary grades we know that the younger children will, as a whole, be the more intelligent. The bright children will have passed on and a number of older retarded children will swell the ranks of the group with low intelligence. We should also strongly suspect the presence of progressive pigmentation. In other words, these younger children would be not only more intelligent but also more blond.

Table II gives the percentage of each pigmentation-group for each successive year.

TABLE II

PERCENTAGE OF EACH PIGMENTATION-GROUP FOR EVERY AGE GROUP

HAIR

Age Group	Boys		Girls	
	Light	Dark	Light	Dark
Nine	0.5	0.0	0.56	1.8
Ten	3.8	2.6	10.1	7.6
Eleven	19.8	14.0	22.0	12.2
Twelve	33.0	27.3	29.3	27.0
Thirteen	24.5	22.4	22.5	29.8
Fourteen	12.2	18.0	10.5	12.6
Fifteen	5.4	11.8	4.6	7.2
Sixteen and over	0.8	3.9	0.5	1.8

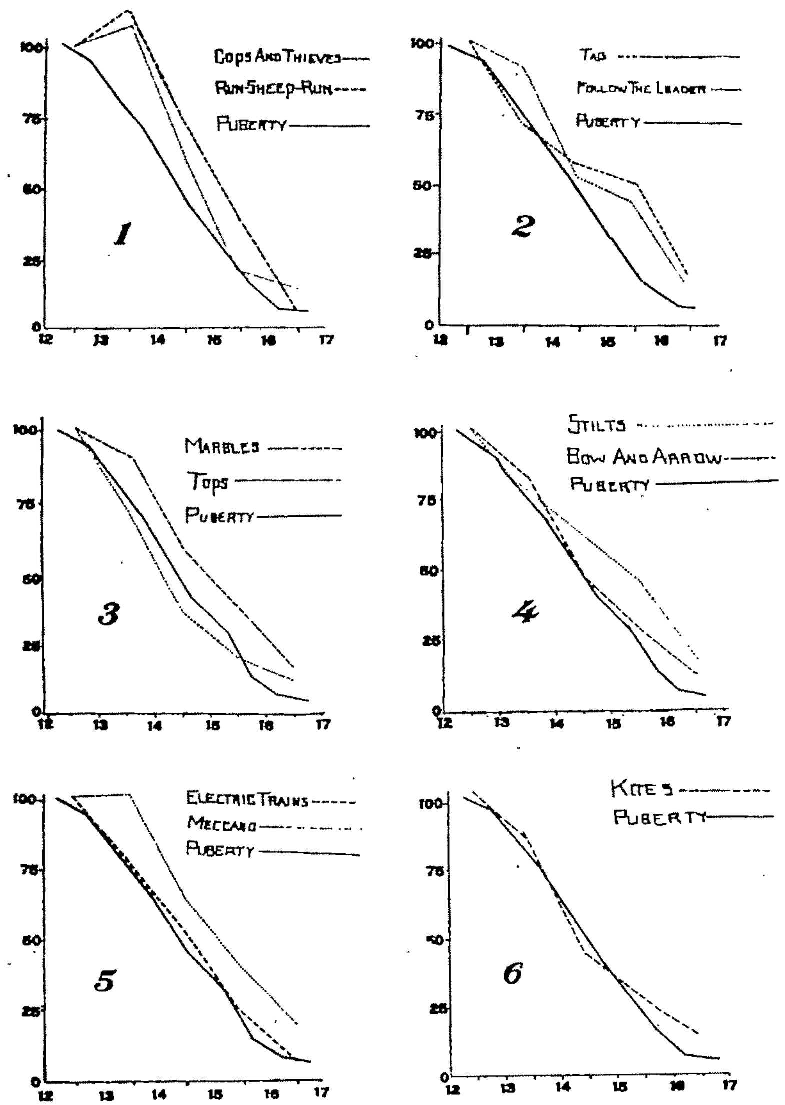
EYES

Age Group	Boys		Girls	
	Blue	Brown	Blue	Brown
Nine	0.3		1.1	
Ten	3.3	0.8	10.8	5.0
Eleven	16.5	13.4	18.2	17.3
Twelve	29.7	36.2	29.8	23.2
Thirteen	26.0	17.6	23.9	31.3
Fourteen	14.0	16.0	10.8	13.2
Fifteen	8.4	10.1	4.0	9.2
Sixteen and over	1.8	5.9	1.4	0.8

The tabulated results show quite clearly a progressive pigmentation within this group and there can be little doubt that this stands at the basis of the positive results.

As a check, let us take another sample of North European children from the school systems of Medford and Revere, Massachusetts. Here we have an entire group at one age, the eighth year. They were given the Dearborn 'A' Group Test within a week of these observations.

Table III contains their median intelligence quotients. It indicates that no pigmentation-class had any superiority in intelligence as measured by this test. These results are typical of those obtained throughout the entire investigation at Medford and Revere. The Otis Group Test, the Primary Examina-



Figs. 1–6. Number of Boys Checking certain Play Activities at the Different Age Groups, expressed as Percentages of the Number Checking at the Age of 12 Years

The age groups are shown on the abscissae, the percentages on the ordinates. The solid line represents the proportion of boys who have become post-pubescent at the different ages group. For the point under discussion is not what proportion of 12-year-old boys are interested in a given activity, say, playing tag, but rather the proportion of those boys who are interested in tag at the age of 12 who will give it up at pubescence. The proportions are presented in Table I.

In the accompanying diagrams the solid line represents the proportion of boys who have become post-pubescent at the specified ages. The data are taken from Crampton's study of 3835 New York City high school boys.[4] The criterion of post-pubescence was the appearance of a kink or twist in the hair of the pubis. This is probably the best feasible criterion of puberty.

The dotted lines represent various activities. These 11 activities—playing 'cops and thieves,' 'run, sheep, run,' 'follow the leader,' tag, and with marbles, electric trains, tops, kites, Meccanos, stilts, and bows and arrows—were selected because they all showed the common characteristic of declining sharply at the age of 12 years. It will be noted that they are predominantly activities of a dramatic, manipulative, or individualistic type.

It is unwise to generalize too freely on these data, but they certainly suggest that the sudden loss of interest in these activities is due to pubescence.

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[4] C. W. Crampton, Physiological age: a fundamental principle, *Amer. Phys. Educ. Rev.*, 13, 1908, 141-154, 214-227, 268-283, 345-350.

the individual variations were too large. Nevertheless certain tendencies appeared. The best way of demonstrating these seemed to be the following. For a certain test, the results from the entire group of 132 *Os* were divided into quartiles.[4] Then for each of the subgroups, for the emotional subgroup, say, the number of *Os* found in each quartile of the entire group was counted, and expressed in terms of percentage of that subgroup. For example, when all the 132 *Os* were arranged in quartiles on the basis of their average speed in the first 10 reactions, 24.2% of all the E+ *Os* were found to be in the first quartile, 30.3% in the second quartile, 30.3% in the third quartile, and 15.1% in the last or slowest quartile. A tendency on the part of *Os* belonging to this subgroup to react quickly rather than slowly would be indicated by a considerably larger percentage in the first than in the last quartile. 'Considerably larger' ought to mean at least larger than the average of all the differences between first and last quartile percentages in the entire series of experiments. Since we had 8 subgroups of *Os* and 5 tests, there were in all 40 differences between first and last quartile percentages; the average size of these differences proved to be 7.95. The difference in the case of the E+ group quoted above was 9.1. Further, the percentages in the second and third quartiles ought to be consistent with the differences between the first and last quartile percentages; thus if the first quartile percentage is notably larger than the last quartile percentage, the second quartile percentage ought not to be notably smaller than the third, if the difference is to have significance. Finally, if a consistent tendency appears in one group, to conclude that it is due to the main characteristic of the group one must be able to find the opposite tendency in the opposite group: if the E+ *Os* have a tendency to quick reactions, this character obviously cannot be related to their emotionality in any simple way unless the E− *Os* show a tendency to slow reactions.

The following tendencies were indicated by figures that fulfilled these conditions:

(1) A tendency for extraverts to make quick reactions and for introverts to make slow reactions to a noise stimulus. Thirty-six percent of all extraverts were in the fastest quartile of the first ten reactions, 24% in the second quartile, 28% in the third quartile, and 12% in the slowest quartile; a difference of −24 between fastest and slowest quartiles. The difference between the second and third quartiles is in the wrong direction, but so small that it is negligible. Only 16.4% of introverts are in the fastest quartile; 21.8% are in the second quartile, 23.6% in the third quartile, and 38.2% in the slowest quartile. As the extraverts and introverts were our largest subgroups, this looks like a true difference.

(2) A tendency for the first 10 reaction-times of emotional *Os* to have larger average deviations than those of calm *Os*. Of the emotional *Os* 30.3% are in the most variable quartile, 27.3% in the second quartile, 21.2% in the third quartile, and 21.2% in the least variable quartile; a difference of −9.1 between the first and last quartiles. This is not large, but the figures for the calm group are more striking. Twenty percent of these *Os* are in the most

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[4] An *O* often belonged in more than one subgroup.

variable quartile, 20% in the second, 20% in the third, and 40% in the least variable quartile; a difference of +20 between first and last quartiles.

(3) A tendency for timid *O*s to make fewer errors in tracing an outline star by mirror-drawing. Of the timid *O*s 37.04% are in the quartile making fewest errors, 29.63% in the second quartile, 11.1% in the third, and 22.2% in the quartile making most errors; a difference of −14.8. Of the fearless *O*s 21.2% are in the quartile making fewest errors, 15.1% in the second quartile, 33.3% in the third, and 30.3% in the quartile making most errors; a difference of +9.1 between first and last quartiles.

The following are less consistently indicated:

(1) A tendency for more timid than fearless *O*s to react very slowly in the first ten. The percentage of timid *O*s in the fastest quartile is 28.5; in the second quartile it is 14.3, in the third 17.9, and in the slowest quartile 39.2; a difference of +11.2. The percentage of fearless *O*s in the fastest quartile is 27.3; in the second quartile 33.3, in the third 24.2, and in the slowest quartile 15.1; a difference of −12.2 between first and last quartiles. The fact that the percentage of timid *O*s in both first and last quartiles is above 25 indicates that this group is here bimodal, some timid *O*s reacting especially quickly and others especially slowly.

(2) A tendency for more timid than fearless *O*s to experience rapid fluctuations of the outline cube. The percentage of timid *O*s in the quartile of most rapid fluctuations is 32.1; that in the second quartile 25, in the third quartile 25, and in the slowest quartile 17.8; a difference of −14.4. In the fastest quartile the percentage of fearless *O*s is only 17.6, but in the second quartile it rises to 29.4; in the third it is 26.5, and in the slowest quartile 26.5; a difference of +8.9 between first and last quartiles.

(3) A slight indication in the direction of McDougall's law that extraverts tend to slow, introverts to rapid fluctuations of the outline figure. For the extraverts the percentages from fastest to slowest quartiles are 20, 28, 20, 32, a difference of +12 between first and last quartiles; but the tendency is not maintained consistently through all four. The percentages for introverts, from fastest to slowest quartiles, are 29, 23.6, 23.6, 23.6; a difference of −5.4 between first and last quartiles, too small to be significant. The only reference[6] McDougall gives to his experimental work furnishes no information as to how he distinguished experimentally between extraverts and introverts and secured that measure of degree of introversion necessary to know "the position of the subject in the intro-extravert scale." Our *S*s, when we measured their degree of introversion numerically by the size of the ratio of introvert to extravert answers, showed no correlation between this degree and speed of cube fluctuations.

One of the questions in the Marston questionary refers to speed of movement, quick movement being taken as an extravert trait. Since our most trustworthy result indicated an extravert tendency to shorter average reaction-times, it seemed desirable to find whether the estimate of an *O*'s speed of move-

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[6] M. Smith and W. McDougall, *The effects of alcohol and some other drugs during normal and fatigued conditions*, 1920. Extraversion and introversion are not alluded to in this paper.

APPARATUS

A DUPLEX MARKER

By RAYMOND DODGE and ROLAND C. TRAVIS, Institute of Psychology, Yale University

This marker (Fig. 1) was designed by Dodge about 1908 for use with a projection chronograph. It writes two non-confusable curves, corresponding to two sets of experimental data, on a single line with a single writing point, and thus guarantees permanent alignment and conserves space.

It has been used to record time and reaction, stimulus and response, and two sets of reactions. It has recorded on various smoked surfaces, on plain paper with ink, and with appropriate optical additions on photographic paper and film. In all these situations and various others it has proved itself a reliable as well as a versatile laboratory instrument.

The essential feature of the instrument consists of a jointed recording lever of two segments—an armature segment *A* and a writing segment *B*. Each segment may be activated independently by its own electro-magnet (*MA* and *MB*) and rotated about an axis in the other segment. The proximal end of *A* is pivoted on an offset from the marker frame. The writing segment *B* is an aluminum bar and has a three-point support as follows: a point offset from the upper core of its magnet engages a cup *a* on the armature to which the lever is attached and provides a permanent point of rotation for all movements of the writing point. This may be combined as an axis either with *b* on the distal end of segment *A* or with either of the two stops at *c*, an offset from the lower core of its magnet or an adjustable stop attached to the frame of the marker.

If an electric current flows through *MA* the writing point is rotated to the left on the axis *a*-*c*. If a current flows through *MB* the writing point is rotated to the right on the axis *a*-*b*. By suitable manipulation of the adjustable stops the excursions in the two directions may be made different in amplitude and consequently non-confusable.

Two adjustable springs *SA* and *SB* furnish the required tension. The compression spring *SA* acts against the proximal end of *B* and serves both to maintain the articulation of the two segments at *b* and to force the armature segment against its stop. The helical extension spring *SB* holds segment *B* against the permanent axis point *a* and forces it against its stop *c*.

The construction makes segment *B* slightly more efficient. This segment is commonly used by us for tuningfork time-lines up to 100 d.v., and for other exacting service.

The marker may be muted by encircling each armature with a small elastic band where it strikes the magnet and stops. This device also decreases the instrumental latency. With optimum rubber cushions our records show a latency to current breaks below one sigma.

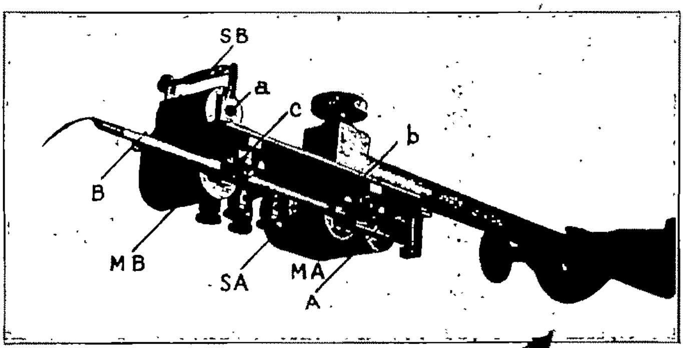
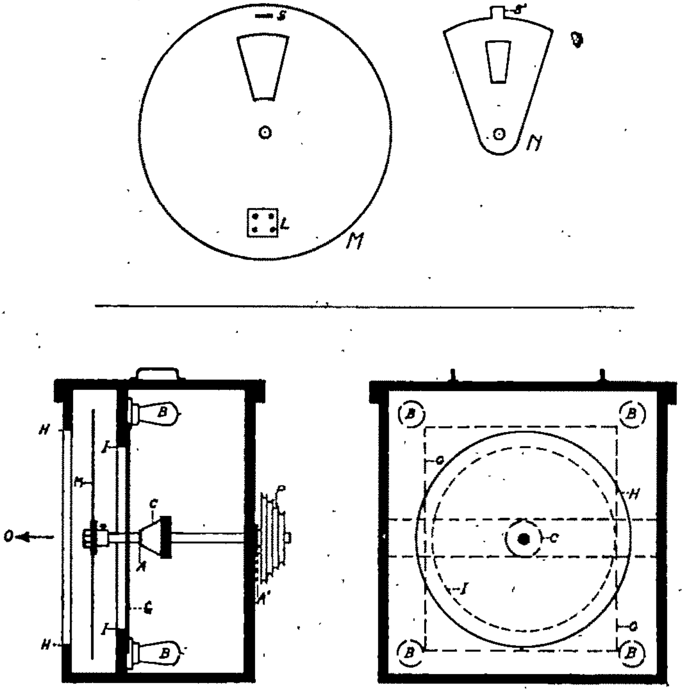


Fig. 1. A Duplex Marker



Side View

Front View

Fig. 2. An Improved Model of McDougall's After-Image Apparatus

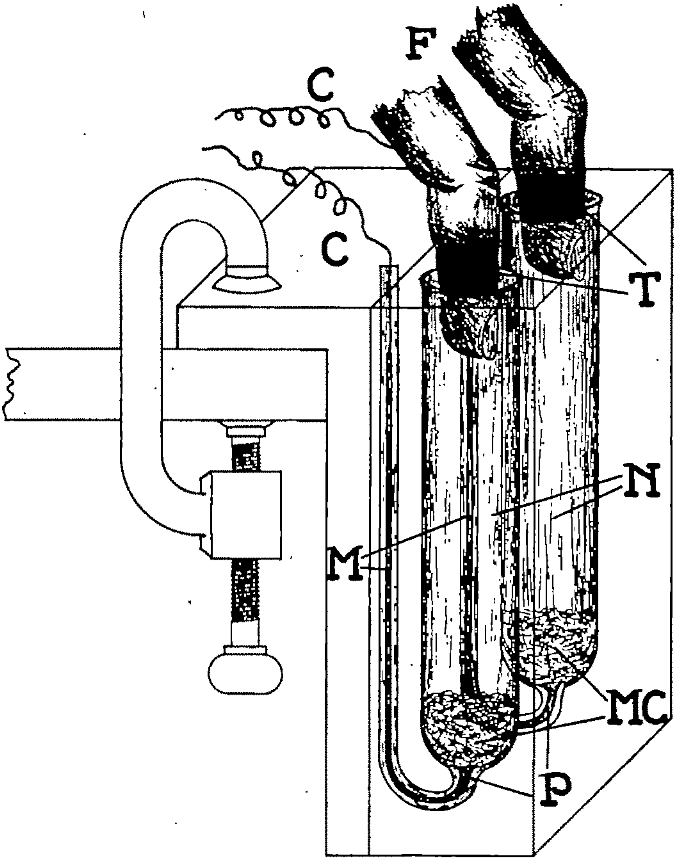


Fig. 3. A Simple Non-Polarizing Electrode

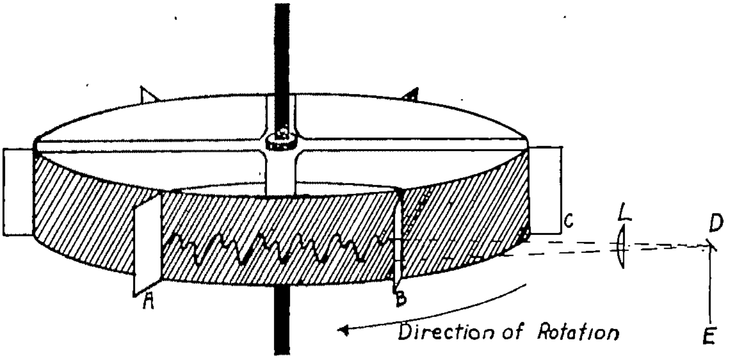


Fig. 4. A New Phono-Projectoscope

This duplex marker is already in use in several psychological laboratories. It is constructed by Mr. C. H. J. Newton of the Wesleyan shop. At present prices it costs about $35.00. It may be wound for either low or high voltage. We use a model that takes about 1.5 amperes current at 3 volts.

AN IMPROVED MODEL OF MCDOUGALL'S AFTER-IMAGE APPARATUS

By W. N. KELLOGG, Columbia University

In adapting McDougall's experiment on the travelling after-image to an advanced course in the psychological laboratory at Columbia, it was found necessary to alter considerably the design of the original apparatus. The model here presented may be regarded, therefore, as a practically new instrument.

McDougall sent light through a narrow aperture in a large rotating disk.[1] By this means the positive after-image appeared to the observer of the disk much like the tail of a comet, so that its stages and details could be conveniently studied. For our uses the apparatus had two serious defects: first, the large screen placed before the disk to prevent stray light from reaching the *O* seemed to call for a separate room to house the apparatus, and secondly, the location of McDougall's gas-light source required a strong support for the rotating shaft and this cast a troublesome shadow on the disk.

To eliminate the screen and the dark-room we used a black portable wooden box, Fig. 2. To remove the second defect and also to permit the installation of electrical connections the internal design was modified as appears in this figure. The rotating disk and its bearings and shaft are set in the square wooden box (ca. 70 cm. x 70 cm.), which is lined with white asbestos paper. The box has a circular opening at the front (Fig. 2, H–H) through which the observer (beyond "O" in the figure) sees the disk. Its two compartments are separated by a square piece of ⅛-in. opal glass (G) just behind the rotating disk (see Fig. 2, I–I). The shaft carrying the disk pierces this glass at the center and is sustained by bearings at A and A′ (side view). The pulley (P) behind the box is connected by a belt-drive to a variable speed 120-v motor. The light bulbs (B)—frosted 25-watt—are arranged to cast an equable light without shadow upon the opal glass. The intensity of light is regulated by the size and number of bulbs and by voltage. The box has protected ventilating openings in the removable top, and is supplied with handles. The disk is fastened to the shaft by means of a special flanged end-piece (Fig. 2), two 3-in. brass washers and a large nut.

In making the disk (see Fig. 2), we used 20-gauge galvanized sheet iron, painted black. The advantages over McDougall's "stout millboard" are obvious. Accessory sectors with various small apertures (*e.g.* N, Fig. 2) can be fitted over the larger opening in the main disk and the sector is held in place by the insertion of its projecting arm S′ into the slot S in the disk. A lead counter-weight, L, is bolted to the disk.

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[1] W. McDougall, The sensations excited by a single momentary stimulation of the eye, *Brit. J. Psychol.*, 1, 1904, 87-92.

A NEW PHONO-PROJECTOSCOPE

By MILTON METFESSEL and JOSEPH TIFFIN, University of Iowa

The commonest device for projecting sonorous wave-motion for visual inspection or demonstration has been the Koenig rotating mirror, in use by the physicist for over a half-century.[1] The apparatus herein described seems to us to be an improvement upon this convenient device. It consists of a horizontally rotating drum about 2 ft. in diam. and 7 in. in height. On the circumference of this drum are placed 6 projecting plates, each plate forming an angle of about 30° with the radius of the drum at that point. The plates are of the same height as the drum and project about 2½ in. outward. On each plate is glued a piece of white cardboard the same size as the plate.

The sound-wave (or any oscillatory motion which it is desired to project) is caused to actuate a beam of light by means of an optical lever. This beam of light is focused from one side of the apparatus upon one of the white cardboard plates from such an angle that the beam will continue to strike this plate through one-sixth of a revolution of the drum, *i.e.* until another plate has arrived to receive the beam. Retinal lag operates to enable the observer to see the waves which have been projected while the drum has revolved this distance. For continued observation of the wave it is only necessary to rotate the drum at such a speed that the time taken by any plate in traveling the distance between it and the next plate is less than the period of retinal lag. In practice we have found a speed of about 120 r.p.m. to be about right.

The operation of the device can be seen from the figure (Fig. 4). As the drum revolves, the beam E, reflected by the optical lever[2] whose fulcrum is at D, is carried by plate A until plate B arrives and intercepts it. Plate B then carries the beam until plate C arrives to take it, and so on. L is a convex lens whose focal length is about 3 ft. It is placed between the optical lever and the drum. The use of this lens of long focal length permits the fulcrum of the optical lever to be set far enough away from the drum so that the amplitude of the projected wave is practically the same over the distance where it is visibly projected. The circumference of the drum, shown by the shaded area in the figure, is painted black to cause a greater contrast between the projected beam and the background on which it is seen.

The advantage of this projectoscope over the old Koenig method is twofold. First, if any one of the Koenig mirrors does not form an angle of exactly 180° with the rotating axis, the wave will change its axis every time a mirror reflects it. The apparatus here described obviates this trouble simply, for the beam is always seen exactly where it strikes the white cardboard. The second advantage is that the light reflected from the optical lever is used much more economically than formerly in that none is lost in a second reflection. Using a Dorsey phonelescope in connection with this apparatus we are able to project sound-waves so clearly that they are easily seen by an entire class in broad daylight.

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[1] R. Koenig, *Phil. Mag.*, 45, 1873, 1-18, 105-114.

[2] A number of these are on the market, probably the most popular being the Dorsey phonelescope, manufactured by H. G. Dorsey, Washington, D. C. (Cf. *J. Opt. Soc. America & Rev. Scient. Instr.*, 6, 1922, 279.)

THE CINEMATOSCOPE AS A MEMORY APPARATUS

By W. B. PILLSBURY, University of Michigan

Miles and others have reported uses of the moving picture projector as a tachistoscope and related instruments. I have found it possible to use one as a demonstration memory apparatus, and it might be used for research. I use a 16 mm. amateur instrument. The syllables may be written in India ink on a transparent leader strip. It is advisable to write them on at least two, and better three or four successive exposure spaces, for one exposure does not give a clear impression. Better still, the syllable pairs for learning may be printed or made of pasted letters and then photographed. A turn or two should be given to each pair. Then the single test syllables may be taken one at a time.

In showing them the series of paired syllables may be cut from the film, the two ends of the film pasted together and if the exposure apparatus is turned by hand, the series can be given as many exposures as necessary to learn. When learning is complete, the single first syllables may be shown one by one and the number remembered determined. In a class, many of the laws of learning may be demonstrated.

The cost of one of the standard patterns is no more than is asked for a good memory machine, and if the syllables are printed by hand the cost of use is negligible. The many other uses of the instrument more than justify its purchase.

Volition is recognized, but is characteristic only of highly organized acts, not of the elementary processes which compose them. Thus volition does not ordinarily initiate but merely regulates the on-going activity. Higher integrations cannot be understood without recognizing the tremendous influence which society exercises on human behavior through language and conventions.

The treatment of perception and of the higher intellectual processes is conventional. We think in terms of words, and symbolic attitudes. "What does it mean to comprehend? To comprehend is in reality to adopt an adequate attitude, mental or exteriorized." Language being the necessary instrument of thought, it is doubtful whether animals advance beyond motor trial-and-error. Man, by the use of creative thought, economizes experience. Attention is defined as a concept which merely "expresses the existence of an 'interest' and of a correlative orientation of activity and attitudes, an orientation defined and unified in the sense that the organism is entirely subordinated to the direction imposed by the dominating interest." Certain individuals lack attentive capacity due to the feebleness of their interests or their lack of mobilizing energy. I see no justification for the author's dogmatic assertion that peripheral theories of attention "confound the feeling of being attentive with the attentive process itself" and that the cardiac, vascular, respiratory and muscular modifications are overflow phenomena without utility. This position, in advance of proof, is no longer justified, in view of the recent discovery that muscular tension increases the efficiency of mental processes.

The chapter on work and fatigue, and the discussion of effort are distinctly modern in tone, as are the discussion of growth of intelligence, personality types, and mental profiles. But in the author's treatment of race and sex differences, it strikes one as strange to hear it asserted unconditionally that "the masculine type possesses a more coherent logic, a more marked ability for invention and intellectual creation, etc., than the feminine;" and that, "between white, yellow, and black races there are differences in type of intelligence and character, which exist even when training has been identical."

Incidentally, the author shows an intimate knowledge of recent American psychological literature, and makes frequent reference to American authors, even though the rule of the book is to omit all references, in order to conserve space. The book is well done, and in the main expresses a sane and critical viewpoint.

University of Chicago ARTHUR G. BILLS

The Future of an Illusion. By SIGMUND FREUD. Translated by W. D. Robson-Scott. No. 15 of The International Psycho-Analytic Library, edited by Ernest Jones. The Hogarth Press, London, 1928. Pp. 98.

This atheistic essay possesses all the serenity, kindness, and temperate reasonableness of a valedictory, although the controversial nature of its subject has always been, for less well-balanced writers, a temptation to dialectic excess and fanaticism. In phrases marked by simplicity and reserve, and unmarred by a self-conscious expression of disdainful superiority, Freud argues first that religion is necessarily a body of dogmas that hamper the free exercise of our rational faculties, which are after all our only resources, and secondly that it is a nightmare of untrained minds, an intellectual error unavoidable in the pre-scientific infancy of mankind, but to be outgrown as soon as possible and replaced by reason.

What path does he take to arrive at this conclusion? Two widely diffused human characteristics are responsible for the fact that the organization of culture can at present be maintained only by certain measures of coercion: that is to say, men are not naturally fond of work, and arguments are of no avail against their passions. Therefore, the problem of civilization has been to diminish the burden of sacrifices imposed on the instincts, to reconcile men to those that must necessarily remain, and to produce a compensation for these. All would be well if the leaders were people of superior insight who had attained the height of mastering their own instincts. But the danger exists that in order not to lose their influence they will yield to the masses more than these will yield to them. Indeed, Russian mysticism has come to the sublime conclusion that sin is indispensable for the full enjoyment of the blessings of divine grace, and therefore fundamentally pleasing to God. Also, that the greatest sinners make the greatest saints is an old doctrine. Hence, an acceptable form of guidance is not to be found in the ruling class.

What remains, then, is to create a majority that will have a proper respect for reason. Having experienced the benefits of culture early in life, they will feel it to be their very own possession and will be ready on its account to make the sacrifice in labor and renunciation of satisfaction of the instincts to the degree necessary for its preservation. They will be able to do without coercion and will differ little from their leaders. If no culture has so far produced this condition, it is that no culture has yet attempted to influence men in such a way. One must ask the question, then, whether and to what degree it would be possible for another cultural milieu to efface the two characteristics of human masses that make guidance so difficult. In other words, it becomes a problem concerning the extent of man's capacity for education. Thus Freud makes his transition from the economic to the psychological plane.

The entire problem is, then, what must be done to reconcile men to and recompense them for their unavoidable sacrifices on behalf of the preservation of civilization. Hitherto religion has found a solution in threats against disobedience and the promise of a later opportunity for the gratification of wishes, as a reward for obedience. Now the nature of this solution must be examined. In a manner somewhat suggestive of Tom Paine, Freud touches upon the literary problem of the authenticity of these promises as given in revealed religion, and of course rejects them as patently unauthentic. Then he examines the promises themselves in order to discover their psychological determinants. In order, he says, to preserve culture, with which mankind defends itself against the crushing supremacy of nature, man must renounce the free play of his instincts. But the desired wish-gratification which religion offers as recompense for this renunciation is only a typical illusion in that it is not the residue of experience or the final result of reflection, but rather a fulfilment of the oldest, strongest, and most insistent wishes of mankind. The secret of its strength is the strength of these wishes. It betokens a tremendous relief for the individual psyche. The part played by the wish in this error is very clear. The illusion need not necessarily be unrealizable or incompatible with reality, but to reason the fixation of habits by pleasure, rightly gives up the hope of getting any light on the problem from the mere fact that pleasure is associated with certain physiological activities and pain with others. Such a position, she points out, commits one either to the point of view of psychophysical parallelism or to that of interaction. The fact that recency cannot be a selective agency in learning, because it could only tend to fix the errors produced at any time or at any stage in the learning, is explicitely recognized; but the difficulties of explaining how persisting erroneous acts are finally eliminated are not given their due attention. On the whole the book is well organized and is sure to interest intelligent readers in psychology from a genetic point of view which is most essential for the solution of problems in human behavior.

J. P.

Manual of Psychiatry. Edited by AARON J. ROSANOFF. Sixth edition, revised. New York, John Wiley & Sons, Inc. 1927. Pp. xvi, 697.

Every psychologist who has even occasional need for a guide to psychiatry must long ago have discovered the indispensable values in Rosanoff's *Manual*. Hence a new edition of that work is appealing. Naturally one looks first for the new material, and notes with pleasure a chapter on the psychoses associated with lethargic encephalitis. There is also one on pellagra effects. The material on personality has been brought together in a special chapter and there is a new chapter on the disorders of childhood. The Hoch-Amsden guide has been added and also tables of height-weight norms and the Inglis tables for the computation of intelligence quotients. To the presentation of association reactions there is added a list of juvenile reactions taken from the Woodrow and Lowell study. Aside from these and some other additions there is much rearrangement and the occasional inclusion of new material, although these changes are not sufficient to be seriously confusing to those who have familiarized themselves with the organization of the preceding edition. The index now includes definitions of technical terms. To the list of contributors has been added the name of J. Harold Williams.

In the seven years since the fifth edition there has been a voluminous addition to the literature of psychopathology and psychiatry. The reviewer therefore, turned quite naturally to the discovery of what in that literature had been deemed worthy of inclusion, of how the new had influenced the old, giving his attention of course to those sections and topics of most interest to psychologists. The chapter on hallucination has been expanded by the addition of a couple of paragraphs giving recognition to psychological interpretations, and it mentions especially the psychoanalytic, referring the reader to the chapter on psychoanalysis. The interested reader will discover, however, that the psychoanalytic theory of hallucination received no detailed presentation. The work of Jaensch and the eidetic school and its bearing on the interpretation of hallucination was not included. The chapter on mental deficiency has been much expanded and shows the influence of the newer literature, but the critique of the inheritance studies by Myerson is not included. To the chapter on epilepsy there has been added a paragraph on migraine with one case and one reference. The statistical presentations have been brought up to date and the presentation of hygienic measures considerably expanded. There is but one entirely new reference, this being that on the ketogenic diet. The extensive literature which has come from the many studies by both the somatic and the psychogenic schools is omitted. The chapter on dementia praecox remains much as in the previous edition, with the same emphasis upon heredity as a causative factor. The term introversion and the literature which it represents found no place. Adolf Meyer is quoted at length but the psychoanalytic interpretations are omitted. In the presentation of the manic-depressive psychoses there appears a brief mention of "fixation of libido" as a possible pathogenic factor. References to endocrinological studies of this group do not appear. The tenor of the changes in the chapter on paranoia is indicated by the change of title from paranoia to paranoic conditions.

There is a new chapter on the theory of personality. It is written from a very practical and almost purely psychiatric point of view. The basis of classification is that which the editor has already made well known. His four forms are well presented by examples and descriptions, although it is notable that the epileptic personality is presented without a single reference to the many presentations of that personality by L. Pierce Clark. All but three of the references bear dates prior to 1920 and there is no mention of the groups or schools represented by Jung, Prince, and Hinkle. The literature on the relation of personality to body types is also omitted.

The chapter on psychoanalysis has been little altered. There are still the long quotations and the abstracts from the earlier literature of the subject. The later literature is not noted. Even at the close of the chapter where proper apologies are made for the impossibility of condensing psychoanalytic literature into so short a space and where the reader is referred to original sources for a better knowledge, the references given are nearly all prior to 1910, and only one is as late as 1918.

Mental disorders of childhood are presented in a new chapter which should prove serviceable, especially to physicians. To the psychologically trained reader the influence of the California group will be evident. The presentation of technique for the examination of aphasias still remains that of Meyer. Head is not mentioned.

In the mind of the reviewer the new edition does not manifest as thorough an examination and digest of the literature which has appeared since the publication of the fifth edition as the previous editions did of the literature extant. Either that or the appraisal of the later literature differs greatly from that made of it by the reviewer. But readers unfamiliar with the Rosanoff *Manual* must not interpret this as detracting from the value of the book as a whole, which the reviewer still believes to rank among the few most serviceable.

University of Oregon EDMUND S. CONKLIN

Psychologie de l'Art: Essai sur l'Activité Artistique. By HENRI DELACROIX. Paris, Félix Alcan, 1927. Pp. 481.

The author does not pretend that this book is a philosophy of art or even a psychology of art. He calls it *Psychologie de l'Art* because, as he says, the title is convenient and because it indicates his intentions.

In the first part, Delacroix discusses the most general conditions of esthetic activity. He approaches the world of art through the world of play. The authority. With the attainment of pubescence, however, the *S*s revert to their former social relations with their governess, and develop in addition special leaderships.

A final chapter is replete with incidental observations confirmatory of previous research on group coldness toward newcomers and intra-group contests for superiority. A social relation which might have merited consideration is that which crops out in the references to home and parents as found in the writings and drawings of the *S*s. A bibliography completes the work.

Johns Hopkins University GREGORY J. SCHRAMM

Problems of Social Well-Being. By JAMES H. S. BOSSARD. New York and London, Harper & Bros., 1927. Pp. xvi, 654.

Social well-being is a state of society whose individuals are in a condition of well-being. Conditions for social and individual well-being are identical. It is probably easiest to make the meaning of well-being clear by stating what it is *not*, called by the author "ill-being." Formerly ill-being was treated under the three D's—defectives, dependents, and delinquents. The expression is too long and still it fails to be discriminating enough as to the insane, the feebleminded, paupers, inebriates, the diseased, blind, and deaf. At least 15 percent of our population have lived in recent years at the poverty level; non-effectiveness due to ill health is between 5 and 6 percent; among those low in intelligence at least half a million are in need of institutional care. The author estimates that the annual cost to this country for care of ill-being is $2,000,000,000. Must this stupendous expenditure continue indefinitely? The author emphasizes the essential unity and interdependence of various social factors, the basic factors being, in his opinion, economic, physical, and psychological; and he does not stress the formulation of so-called sociological principles, being sceptical concerning the identification of such principles. The book contains a wealth of data from reliable sources on economic, medical, and other aspects of social well-being, which lie outside the field of our present interest—though by no means to be neglected by psychologists—and which therefore we must pass over. An interesting account of the progress of medicine is given, from the period of early superstitions to the modern view of germ infection and the methods of treatment and prevention, including an account of the modern public health movement. The causes of accidents are traced from their mechanical through their more strictly psychological factors—fatigue, excessive speed, professional daring, inexperience at the beginning of one's employment, youth, and lack of training. Finally, the effects of limitation of intelligence are briefly and very inadequately considered. This is partly accounted for by the statement that intelligence factors fail to get due recognition in the industrial safety movement. But for that very reason, would it not be well to marshall the facts relating to the reduction of accidents in certain fields of industry, especially in connection with the handling of electric cars and automobiles, by the elimination of workers of low intelligence and those having certain sensory defects, as well as by better placement according to specific qualifications? We are told that perhaps a majority of workers today believe that they are fore-ordained to die on a certain day, and that until the arrival of the very hour that they are destined to go 'the Lord will protect them.' The removal of such attitudes must in large part be the work of a sound psychology.

Part III, covering 137 pages, is devoted entirely to mental hygiene in its relation to well-being. A full and, on the whole, very satisfactory treatment of mental deficiency, based on the work of leading psychologists in this line, is given in two of the three chapters of Part III. Leading investigations in the heredity of mental deficiency are considered, the author being careful to indicate that as yet many problems regarding the causes and heredity of feeble-mindedness are still unsolved. A short account of the historical development in the treatment of the mentally deficient is given, attention being paid to the relative costs as well as to effectiveness. Identification and diagnosis with registration of all recognized defectives is urged, as well as proper training and care. Such a scheme would reveal the families and individuals most likely to transmit feeble-mindedness, and therefore the ones most needing segregation. Steralization methods have failed to meet the expectations of the advocates of this plan, this being due in good part to public disapproval and hesitancy on the part of many informed persons because of uncertainties in diagnosis and prognosis. Such drastic action can apply only to very extreme cases at best. On January 1, 1923, there were 267,617 patients with mental diseases in hospitals for their care in this country, but this number would be much greater if those who are paroled and those who ought to be in hospitals were included. A chapter is devoted to mental diseases and one to problems of mental hygiene, the author following closely in these, as in other chapters more or less technical to different sciences, the several contributors cited, often quoting at length.

The book gives a broad and sound view of the several aspects of social well-being, a survey that is worth while to psychologists.

J. P.

Das soziale Verhalten von Mädchen während der Reifezeit. By LUCIA VECERKA. Part II. Quellen und Studien zur Jugendkunde. Edited by Charlotte Bühler. Jena, Gustav Fischer, 1926. Pp. 74.

The author assigns to Englishmen and Americans the pioneer laurels in this sphere of psychology. The study is a continuation of the advances made by Schjelderup-Ebbe, Katz, and Reininger. The investigator has had thirteen years experience with girls and has carried on this study in a systematic way for one year. The *S*s range from 11 to 19 years, and are taken from schools at the secondary and normal-school level. They number in all 1421. The dominant method of inquiry was by questionary, which was supplemented by the author's observations.

A chapter on the basic relationships of the period, a second presenting the characteristics of a class as a group, and a third giving detailed consideration to each of these 26 class members, contribute much to the thoroughness of the work.

In a section on the arrangement or structure of the class as a group, the actual position of each member is ascertained in regard to leadership and class achievement. The data were acquired by an ingenious questionary. From in musical education. It should be said that the author is himself a musician of psychological training. Questions and exercises are given at the end of the book for each chapter, and a bibliography of four and one-half pages, but not quite up-to-date so far as American developments are concerned, is added.

The treatment of musical training from the teacher's standpoint seems to the reviewer rather sound psychologically though it often assumes a somewhat dogmatic tone, going beyond what is yet scientifically established. While this book presents no new data on psychological aspects of acoustics, it is extremely valuable as suggestive of fields for further investigation.

J. P.

La pensée. D'après les Recherches Expérimentales de H. J. Watt, de Messer et de Bühler. By Albert Burloud. Paris, Félix Alcan, 1927. Pp. 192.

This work gives an extended review of the various publications by the men mentioned in the title bearing upon the nature of thought and related processes, with criticisms of them. The original work needs no further comment. Burloud accepts the method as valid against the criticism of Wundt and others.

The results are discussed under three main heads: Is there imageless thought? What is thought as a fact of consciousness and of what elements is it constituted? What is thought as activity? The first question is answered rather more in the negative than in the affirmative. The author concludes that while we may have cases in which we are aware of thinking but can find no images, still the relations, intentions, attitudes and what not, that are not themselves imaged would not arise in default of images. They may serve to give to the image the character of thought, but the images are implied.

The elements of thought, it is asserted, are not clearly distinguished or logically classified by the Würzburg school. They seem to reduce, on the one hand, to the tasks, intentions, or what not, that give direction to the course of thinking, and on the other hand they reduce to 'feelings' of one type or another. Task or direction is not necessarily conscious. If we accept it as an element of thought it can be regarded indifferently as attitude or intention. It is at once a scheme or plan and a directing force in consciousness. As feeling, the somewhat trite suggestion is made that it is necessary to distinguish the affective sentiments and intellectual sentiments. The awareness of attitudes, etc., fall in the latter class.

The nearest approach to an answer to the question of what constitutes the active force in thinking is found in Watt's discussions of the task in its various applications. Even to this question Watt does not give a specific answer. Task is a dynamic effect of consciousness itself. It is not shown what relation if any it has to physical processes or in what sense it is dynamic.

The defects grow from the virtues of the works, that they seek an empirical examination of the most difficult problems of psychology. They show that observation needs to be supplemented by thinking, the study of the completed process by consideration of its evolution. The book gives a good summary of the work of the school, although the criticism strikes one as on the whole superficial.

University of Michigan W. B. Pillsbury

An Experimental Study of the Mental Processes Involved in Judgment. By B. P. Stevanović. London, Cambridge Univ. Press, Brit. J. Psychol. Mon. Suppl., 12, 1927. Pp. 138.

This is an analytical study of the mental processes involved in judgment. The first part is preliminary and reports the learning of thirty-five pictures by 5 *Ss*. The pictures were divided into seven sets, each set of five pictures having a characteristic color and a meaningless name, as bogel, pimef, etc. printed above the pictures. Each *S* learned the pictures and their names "sufficiently well to know them as material for the later 'completion experiments,' and not quite so well as to make the completion too easy and automatic." This criterion gives no assurance that all *Ss* learned the pictures equally well, a fact which may have produced many of the individual differences found later on in the study. The second part of the experiment consisted in completing eight different kinds of sentences in the form of propositions, as "The first bogel is ......" "Some pimefs are ......" The *Ss* completed each sentence by memory as quickly as possible, while *E* recorded reaction-time and galvanic readings for every judgment. Two sets of introspections were taken, one with learning and one with the completion of the sentences. Analysis of the former introspections shows that the pictures go through about eight different stages in acquiring their final meanings. The sentences when completed were classified into individual, particular, universal, and general affirmative, and into universal, particular, and individual negative, according to the kind of judgments involved. The second class of introspections showed that the subjects of negative and particular judgments tend to be present as imageless knowledge more frequently than those of affimatives and universals. "A character obtained by associative reproductive activity is not accepted without verification." In general affirmative judgments tend to involve analytic processes while negative judgments tend to be synthetic. Changes in bodily resistance seem to accompany 'conation' but not 'volition.'

Unfortunately the results are couched in terms and logical categories which can hardly be non-influential in their determination, and interpretations are in terms of a special point of view not very common in America. The writer seems to regard 'conation' and 'volition' as arbitrary influences rather than aspects of behavior to be explained. Spearman's neogenetic principles are considered as ultimates, and the use of these conceptions complicate and vitiate results and conclusions, making their objective use by psychologists impossible. Because of this, many of the author's explanations seem to the reviewer like saying that something happens because it happens. The experiments, however, have several suggestive and interesting aspects.

Peabody College C. W. Telford

Applications of Clinical Psychology in Hawaii. By Marjorie E. Babcock. Honolulu, Mercantile Press, 1927. Pp. 82.

This initial Research Publication of the University of Hawaii, written as a master of arts thesis, is a very interesting piece of work. The first part of it consists in an ethnographic, social and economic bird's-eye view of this polyglot territory. There follows a concise but fairly comprehensive review of the studies of previous workers in clinical psychology, in comparing racial groups.

appeal of Paul of Tarsus lay in his epileptic personality. Mystical and hysterical dream-states alike, are "symbolically mediated indentification with the mother —a narcissistic and homosexual activity" which in the case of the mystic, because of his abhorrence of sex intercourse, allows him his pretense of sex abstinence.

In the sixth chapter Moxon asserts that human fellowship is narcotic in its loosing of primitive impulses and its inhibiting of intellectual activity. The following chapters treat Freudian Criminology, Couéism, and the popular resistence toward the enlightenment of children on sex topics. There are included a psychoanalytical survey of the development of the libido of Frederick Nietzsche, and a critical essay on Freud's death instinct and Rank's libido theory.

Throughout the volume the author has given ample evidence of his faith in Freudian interpretations and methods and of his interest in Rank's "epoch making work on the Trauma of Birth" which has given a "psycho-biological foundation for Freud's findings." He believes, as stated in the preface, that these discoveries, when based on personal experience, give "a degree of control and direction of impulse beyond any other technical means hitherto discovered by man." The many dogmatic statements may be attributed to the intensity of this faith. The volume should be of interest to those who study religion and religious experiences from the psychological aspect as well as to the psychologist of more general interests.

Boston Psychopathic Hospital C. H. KIMBERLEY

How to Succeed in College. By W. F. BOOK. Baltimore, Warwick & York, Inc., 1927. Pp. 192.

This volume presents the results of an investigation of the capacities and adjustments of college students, and of some efforts to improve their attitudes and methods of work. Data were secured by means of an intelligence test, two reading tests, a questionary and a weekly time table filled out by freshmen under the supervision of their instructors during a regular class period. Results are reported for more than 3400 students, over 3000 of whom were freshmen.

Although girls are found to have less endurance than boys, the average time spent in study per assignment is 2.30 hours for girls as compared with 2.11 hours for boys. It should be noted, however, that the sole criterion of endurance consisted in students' answers to the question: "How long can you continue to work on a difficult problem before you get too tired to do efficient work?" Students who do some work for self-support make slightly better academic records than those who do no such work, although their average intelligence-test score is a little lower.

The records of students who were given a course in "How to Study" are compared with those of other comparable students. The average number of credit points earned by 43 freshmen enrolled in this course exceeded that of the entire freshmen class by 30 percent during the first and by 49 percent during the second semester, though their average intelligence rating was not superior to that of the entire class.

Special training in reading was given in the "How to Study" classes. This training was preceded and followed by tests of reading ability. Book reports an improvement of 102 percent in ability to read as a result of such training. One feels a little uneasy about this flattering result, however, when it is observed that calculations of reading ability were based on the time required to read a standardized assignment, each student reporting his own time.

The volume contains much information which should be both interesting and useful to college administrators, instructors, and students. To the casual reader its value may be somewhat impaired by an occasional show of disregard for the grammatical conventions.

Kansas State College J. C. PETERSON

The Psychology of Personality. By ENGLISH BAGBY. New York, Henry Holt & Co., 1928. Pp. viii, 236.

This volume deals with common emotional disorders. Those who are interested in the application of psychology to the broader problems of human conduct will certainly wish to read it.

The psychology of personality as it is treated in this volume involves for the most part a consideration of persistent emotional processes. As fundamental to the explanation of these processes in the individual, two concepts are used. The *activity level* of an individual varies from the rapid energy expenditure of excitement to the slow energy expenditure of depression. The internal conditions which at least in part alter the activity level of the individual and determine the intensity of reaction to external stimulations are characterized as *tensions*. It is pointed out that tensions may have various physiological causes. The notion of tension, however, has proved itself valuable in understanding and treating abnormal personality traits.

The conditioning of emotional reactions, the recall of repressed thoughts, worry, defense reactions, inferiority complexes, rationalization, day dreaming, and hysterical traits, are all considered and illustrated by a wealth of pertinent and interesting case material.

The general principles of treatment are clearly outlined. The basic clinical procedure of the book may possibly best be suggested by characterizing it as a combination of the behavioristic and the psychoanalytic methods. In order to secure coherence certain materials that might have been considered in a book such as the one under review are omitted. For example, the techniques for the detection of the repressed ideas are said to be the association method, dream interpretation, and direct questioning, but the technique of hypnotism is not explained. Even in presenting Dr. Morton Prince's "Bell Tower Case" the detection of the suppressed memory is attributed to a "special technique," but automatic writing is not mentioned. The last chapter contains a number of excellent cases chosen to illustrate the general thesis of the book. As the author himself well says, there has been an undoubted tendency in recent years to neglect purely descriptive studies in psychology. In this volume much has been done to make clear some of those phenomena of mental life which can be understood only when the everyday life of the individual is observed over long periods of time.

Brown University LEONARD CARMICHAEL

a basis for criticism is difficult to attain. When we add that Draghicesco regards this doctrine as having its inevitable culmination in religion it seems far removed from psychology as a technical discipline within the field of the natural sciences.

Indiana University J. R. KANTOR

A Sociological Philosophy of Education. By R. L. FINNEY. Introduction by W. C. Bagley. New York, The Macmillan Co., 1928. Pp. xi, 563.

This is a theoretical discussion of education from the sociological point of view. Its main interest to the psychologist is the uncritical use of terms with which he has to deal. "Social mind" is used "to mean that body of mental content which is held in common by members of the group, and through the use of which group action is made possible." The obscurity of this social mind is only increased when we read that in social readjustments series of acts occur "in the behavior of the social mind." Much is attributed to imitation. The real processes in learning are much obscured, and imitation is made a general formula for most uniformities in conduct. Psychologists will be particularly interested in the chapters on "The Blight of Extraneous Motivation," "Followership and the Duller Intellects," and "Educational versus Economic Determinism." The author accepts the general results of psychological tests, but holds that the lower mentalities act mechanically and in the main largely like the higher mentalities, through imitation and suggestion effected by means of catch phrases. Thus the reasoned behavior of the intelligent individuals is similar, generally speaking, to the rote behavior of the dullards. It is hopeless to teach the latter to think for themselves, so the intelligent must think for them and "drill the results, memoritor, into their synapses. For the dullard it is that or nothing." The psychologist who reads in this book of the various "insights of social psychology," a phrase that occurs frequently, cannot but sense the danger of the "psychologizing" of the sociologists. When it becomes so easy to 'explain' the various complex forms of social behavior merely by the superficial use of psychological terms, it is certain that not only 'dullards,' but many misguided students, will fail to see the need of delving experimentally by more laborious methods into the problems of psychology. The plausibility of many interpretations made by such writers only increases the deception, and delays the progress of real science. J. P.

The Psychology of Play Activities. By H. C. LEHMAN and P. A. WITTY. New York, A. S. Barnes & Co., 1927. Pp. xviii, 242.

The authors define play, with certain reservations, as the activities in which the individual engages "just because he wants to."

Several thousand children in the elementary schools of Kansas City, Missouri, were given check-list questionaries concerning the "play" activities in which they had engaged during the "past week." Data on the National or Terman intelligence test scores of these children were secured. Perhaps the most noteworthy finding is that "with increase in mental age, there was a tendency for the children to engage in smaller numbers of plays and games of a social character."

Also of interest are the following conclusions: "The data give evidence of a consistent tendency for the dull or retarded school child to evince an unusual interest in games in which others take part. . . . The dull or retarded child avoids certain individual activities."

Special studies were made of a group of "gifted" children; of the play of negro children; of the sex differences in play behavior; of the ages at which certain forms of games and play appeared, reached their maxima, and waned; and so on.

This study has been carefully and intelligently handled. It is of the utmost importance to anyone working in the field of child psychology, as it is the most complete and thorough treatment of the play behavior of children that has thus far been reported.

Harvard University DONALD SNEDDEN

The Social Sciences and their Interrelations. Edited by W. M. OGBURN and ALEXANDER GOLDENWEISER. New York, Houghton Mifflin Co., 1927. Pp. viii, 506.

The volume consists of 34 articles on the interrelations of some of the social sciences, and their relations to psychology and statistics; contributed by many specialists in the various fields. The work brings out clearly some of the divergent points of view of students primarily concerned with anthropology, economics, history, political science, or sociology. The optimism for the chosen field and the critical attitude towards outsiders are very apparent. This method of placing departmentalized attitudes side by side is very encouraging, and the book should be widely read by social scientists. There are many references to the basic subject of social psychology, some sympathetic and some critical, but all worth while. Psychologists will find the following articles interesting and also stimulating: "Political Science and Psychology" by Floyd H. Allport, "Sociology and Psychology" by L. L. Bernard, "History and Psychology" by Wilson D. Wallis, "Economics and Psychology" by Z. Clark Dickinson, "Anthropology and Psychology" by Alexander Goldenweiser, and "The Social Sciences and the Natural Sciences" by Morris R. Cohen. Some of the writers it must be said show an astonishing unfamiliarity with modern psychology.

University of Rochester HULSEY CASON

The Mental Life. By CHRISTIAN A. RUCKMICK. New York, Longmans, Green & Co., 1928. Pp. ix, 253.

In the first paragraph of this book the author tells us that Descartes wrote the Magna Charta for psychology. Throughout the volume an attempt is made to show that psychology is the science of experience and that it is not a branch of biology or physics. There is virtually no discussion of the nervous system or the response mechanism in the book save for occasional references to the receptor end-organs.

Sensory experience is treated concisely. It is presented in such a way as to be both clear and interesting to the elementary student. Excellent drawings are given of the diagrams which have been prepared to show the relationship of sensory qualities. The visual double pyramid, the tonal bell, the taste tetrahedron, the smell pyramid, and the touch pyramid, are all clearly presented and explained. In the opinion of the reviewer this material has never before been so graphically presented at the textbook level.

On the basis of the consideration of sensory experience a treatment is offered of perceptual, imaginal, and affective experience. Attention is treated as clearness. The higher processes are considered under the respective headings of association, action, the thought processes, and the self.

The volume as a whole represents a remarkably compact and coherent view of the psychology of experience. It should be of value not only to the beginning student but to the general reader who wishes to be informed upon that type of scientific psychology which is grounded in the study of experience for its own sake.

Brown University LEONARD CARMICHAEL

That Mind of Yours: A Psychological Analysis. By D. B. LEARY. Philadelphia, J. B. Lippincott Co., 1927. Pp. 226.

Under the title of this book the author published a series of articles in the *Buffalo Evening News* for a period of four years previous to the publication of this volume. This experience led him to believe that there is need of presenting a popular but scientific statement of the newer knowledge of psychology. The volume, then, is an attempt to lead the general reader "to see through the eyes of the modern psychologists, to get their slant on human nature, to see solutions and plans as they see them, to work with them . . . toward that better life the making of which would alone justify the delving into human depths" (p. 11). Approximately seventy-five pages are devoted to the behavior of the normal human being, and about ninety-five to abnormal behavior. In the former part the unity of the organism is emphasized and the treatment is based largely on the view of drives, instincts or tendencies, and emotions, which are not clearly differentiated. The author begins with the Lockian view of sensations as the basis of all knowledge, which is not made very consistent with the emphasis on drives, and goes up through questions of volition and responsibility. The part dealing with abnormal behavior begins with individual differences, and leans considerably toward the Freudian view. A closing chapter is devoted to wider applications. The psychologist would prefer the use of 'impulses' to that of 'outgoing messages,' and purely on the basis of experiments would disagree with the statement that beauty depends (apparently wholly) on individual experience. The volume, on the whole, is a successful piece of popularizing work, and can be strongly recommended to the general reader.

J. P.

La Philosophie de l'Art Classique. By DRAGUICHA LAPCEVIC. Paris, Félix Alcan, 1927. Pp. 124.

"The purpose of this study," as the author puts it, "is to demonstrate the necessity for a return to the spirit of antiquity, if there is to be any progress in our civilization," since modernism has made of man a being essentially unnatural and artificial as compared with the Greek. The author regards classic art in every age as the medium par excellence for presenting society with normative ideals of humanity which are biologically correct and desirable. The book evinces familiarity with classical culture and contains here and there suggestive *aperçus*; but there is no new argument or new evidence adduced for what is in the main an acceptable thesis. At times the author seems over enthusiastic, dogmatic, needlessly repetitious, over credulous, and even rather commonplace. That the Venus of Melos represents a standard or even the ideal of perfect womanhood, may pass unchallenged; but not all students of esthetics will agree without argument that the Apollo Belvedere is the masculine counterpart of the Aphrodite (or, as Reinach prefers, the Amphitrite), nor that it is the finest product of classical sculpture. Moreover, students of biology will hardly take as a demonstration the belief of the author that the contemplation of this statue by pregnant Greek women resulted in beautifying their offspring, which seems rather a return to the letter than to the spirit of Greek science.

Vanderbilt University HERBERT SANBORN

Selected Papers on Psychoanalysis. By KARL ABRAHAM. Translated by Douglas Bryan and Alix Strachey. Introduction by Ernest Jones. London, The Hogarth Press, 1927. Pp. 527.

This volume includes the more important papers of the author arranged chronologically. They represent the orthodox psychoanalytic doctrine and show quite well the changes which have occurred in these doctrines. The first paper, published in 1907, emphasizes the sexual traumas in children whereas the later papers discuss the castration complex and similar topics.

The book forms a useful contribution to psychoanalytic literature, giving the views of one of the leaders of this group and showing quite well the manner in which his own views have developed as he continued his studies.

The introductory memoir of thirty-three pages by Ernest Jones gives an excellent account of Abraham's life and work and discusses the various contributions which Abraham has made to psychoanalysis.

Boston Psychopathic Hospital KARL M. BOWMAN

The Supplementary Reading Assignment. By CARTER V. GOOD. Baltimore, Warwick and York, 1927. Pp. ix, 227.

The writer attempted to show objectively whether it is better (from the point of view of accuracy and range of information, reproduction of thought, use of materials read in the solution of situations, and retention of ideas) to read intensively a short selection or to read extensively a much larger amount of material. Four topics were selected for reading and parallel groups of subjects were arranged. After each reading these groups were tested by means of objective examinations. In all the situations except the one dealing with the ability to reproduce ideas, the extensive method was found to be superior.

This book is of little value to psychologists but may serve as a reference in certain courses in educational psychology.

Peabody College S. C. GARRISON

On Stimulus in the Economic Life. By Josiah Stamp. Cambridge, Cambridge University Press, 1927. Pp. 68.

Stimulus is here defined as increment in incentive. Care is taken to distinguish increment in incentive from enlarged scope though it appears that this distinction is ordinarily ignored in economic literature. A classification of stimuli according to the character and permanency of their effects is attempted and each type is illustrated by analogies drawn from different fields of scientific inquiry. No quantitative statements concerning the relation of reaction to stimulus are found which can be carried over into the economic field. A plea is made for the discovery and utilization of stimuli which fit in with economic life and lift it permanently to higher levels of productiveness.

Kansas State College J. C. Peterson

Psychological Studies. By Theodor Lipps. Translated from the second German edition by Herbert C. Sanborn. Baltimore, Williams & Wilkins Co., 1926. Pp. 225.

This translation makes available in English Lipp's three valuable classic essays on "The Space of Visual Perception," "The Nature of Musical Consonance and Dissonance," and "The Law of Psychic Relativity and Weber's Law." These essays are too well known to need comment here. It suffices to commend this translation of them as both accurate and in good idiom.

Hobart College Forrest Lee Dimmick

Why We Look at Pictures. By Carl H. P. Thurston. New York, Dodd, Mead & Co., Inc., 1926. Pp. xi, 338, 86 pl.

In the words of the author, the book is intended "as a practical aid to the enjoyment of pictures." Aesthetics, rather than psychology, constitutes the subject matter, though there are psychological discussions of color, form, etc. The book is written for the "average man" and the principles presented are made clear by frequent reference to a group of illustrations of famous paintings at the end of the book.

Geneva, N. Y. Claire C. Dimmick

Le Progrès de la Conscience dans la Philosophie Occidentale. By Leon Brunschvigg. Paris, Félix Alcan, 1927. In two volumes. Pp. xxiii, 807.

In these volumes M. Brunschvigg sketches with a bold and sweeping brush the moving picture of the development of ethical thought and teaching in the eastern European tradition. Starting with Socrates, the author passes in review the principle problems which have agitated philosophers concerning conscience, duty, and the place of reason and intelligence in the domain of correct conduct, down to Bergson and his contemporaries. While these are philosophical volumes they are not without value to the student of psychology who is interested in the general intellectual background in which his science has had its origin and much of its development.

Indiana University J. R. Kantor

Recent Developments in the Social Sciences. Sociology by C. A. ELLWOOD, Anthropology by C. WISSLER, Psychology by R. H. GAULT, Cultural Geography by C. O. SAUER, Economics by J. M. CLARK, Political Science by C. E. MERRIAM, History by H. E. BARNES. Edited by E. C. Hayes. Philadelphia, J. B. Lippincott Co., 1927. Pp. vii, 427.

This book, written by seven specialists, is a comprehensive and critical review of the significant trends in the social sciences. It has a general appeal to the psychologist interested in the intellectual movement called sociology. In the chapter on psychology, however, such medical statements as "the injection of adrenalin is a means of recovery from fatigue," and "low levels of intellectual life are correlated with inadequate functioning of glands of internal secretion alone," should be taken with the same grain of salt that one takes the story that Tom Thumb, the midget's son, ate a pituitary plum and became the Giant that Jack killed.

Boston Psychopathic Hospital S. C. ROTH

The Kingdom of the Mind. By JUNE E. DOWNEY. Introduction by Edwin E. Slosson. New York, The Macmillan Co., 1927. Pp. ix, 207.

This is the third volume of *The Young People's Shelf of Science,* edited by Slosson. It is a presentation, chiefly for boys, of a diversified group of experimentally established facts in psychology. Though a relatively large amount of space is devoted to facts of secondary interest, like the blind spot, cutaneous sensitivity spots, minor illusions, images, etc., the author shows skill in relating these facts to larger human interests. She tactfully and informally leads her young readers into methods and values of taking self-inventories. The book is sound scientifically, and will doubtless furnish profitable recreation to young people and to the popular reader generally.

Peabody College C. W. TELFORD

Die praktische Psychologie im Wirtschaftsleben. Eine systematische und kritische Zusammenfassung des gesamten Gelietes der Wirtschafspsychotechnik. By WILHELM WEBER. Leipzig, Barth, 1927. Pp. 418.

This, the latest of the many German books dealing with industrial psychology, purports to be a thorough treatment of the subject, covering in addition to the usual topics of production, vocational guidance, fatigue, the subject of advertising and selling, and the bearing of industrial psychology on psychology in general and on the various psychologies in particular.

The discussions are copiously annotated with references to experimental literature, though practically all the references are to German works. Citations are made to tests designed for the prognosis of aptitude in about seventy-five occupations.

A section on industrial psychology in foreign countries devotes several pages to work of American psychologists but the sources from which the descriptions are drawn are obviously secondary. The chief topic treated is the threadbare one of psychology in the army. No developments in vocational guidance of a later date than the time of Parsons, 1908, are cited.

The frequency with which the word *Arbeitspsychologie* appears in German publications suggests that an English equivalent might render important service in directing the thinking and experimentation of American psychologists along the line of industrial psychology.

The book reflects very clearly the large amount of activity in this field which is taking place in Germany, and will serve as a valuable index to American readers who wish to know of these developments.

Columbia University H. D. KITSON

Aesthetics of the Novel. By VAN METER AMES. Chicago, University of Chicago Press, 1928. Pp. ix, 221.

We admire art, this book holds, because by empathy it refreshes us. People read novels to gain vicariously what they are yearning for. It therefore becomes necessary to understand human personality if the novel is to be judged as a work of art. Moreover, the reader's character determines the novel he will select, and his reading of the selected novel itself further molds his character. The book is brilliantly written. It may be that it is too brilliantly written. The style of some of Ames' paragraphs is so dazzling that the reader is distracted by ingenious word patterns from the important thesis of the volume as a whole.

Brown University LEONARD CARMICHAEL

An Elementary Psychology. E. D. PHILLIPS. Revised Edition. Boston, Ginn & Co., 1927. Pp. v, 420.

The revisions in this book are not intended by the author to bring it up to date as a scientific book on psychology. Scientific technicalities and most of the content of psychology, too, are omitted. The chief emphasis is on the attempt "to stimulate young people to build their own character and wholesome relation to others." The treatment of psychological topics is general, loose, and no concern is shown for precision; the emphasis is placed on the arousal of interest. The revision includes certain of the recent contributions to psychology from this general standpoint. The reviewer doubts the helpfulness of this sort of book to the better class of college students.

J. P.

Morale et Pedagogie. By PAUL LAPIE. Paris, Félix Alcan, 1927. Pp. 238.

This book is a collection of articles published in the *Revue Pedagogique,* and the *Manuel Général.* This volume and a companion volume entitled "Méthodes et Directions" commemorate the work of Paul Lapie. Lapie's philosophy of education is given in papers including "The immortal flame," "The scientific miracle," "Sophisms," "Meditations on Enthusiasm," "The code of ethics of Humanity." Several papers deal with specific phases of school organization and work.

"An experiment" compares 660 compositions written by students in the Lycées with 856 written in the Écoles Normales. Among the differences in the two systems is the absence of Latin and Greek in the Écoles Normales. The quality of the compositions in the Écoles Normales is superior to that in the Lycées. A comparison of the ages of the two groups shows the Écoles Normales students to be enough older to account probably for their superiority.

North Carolina College for Women J. A. HIGHSMITH

NOTES AND DISCUSSIONS

Methods of Practice Equilibration

Although the student of experimental psychology will sooner or later discover the various psychophysical methods and the various methods for experimentation within the field of memory through sheer exposure to reports of individual researches, we commonly recognize a value in culling these methods from their setting in individual experiments and presenting them together in some sort of systematic arrangement. For some time I have been presenting to my students a similar classification and systematization of the methods commonly utilized in equilibrating the effects of practice. The particular methodological problem here represented has, of course, become increasingly important during these last twenty years, which have seen a wide extension of experimentation dealing primarily and frankly with capacity and performance.

When presenting this inventory of practice equilibration methods to students I illustrate each procedure by the citation of one or more actual investigations. Since the professional psychologist will readily supply his own illustrations, however, I shall omit them in these notes. Nor is much demanded here in regard to the prevalence and importance of practice effects throughout all but the simplest of psychological performances.

I. The Method of Completed Practice

The method of completed practice requires that improvement due to practice in the performance in question has ceased to occur. If this is actually the case the *practice order* in which one or another condition of experimentation is placed can safely be neglected. Insofar as order influences 'fatigue' or 'set' or some other significant variable it cannot be ignored, but so far as we are concerned merely with practice order that factor can be neglected.

Upon first thought the method of completed practice would seem to offer an ideal means of equilibrating practice effects. In pure form, however, it is almost never employed. Two of the difficulties are practical. For most types of performance the actual limit of practice is beyond experimental reach. Even where a level of achievement seems to have been reached, we know that a slight alteration in attitude is likely to bring about a resumption of improvement from practice.

A second difficulty with the method of completed practice is that even in its merely approximate forms it can be used, as a rule, only with a small number of *S*s. Many repetitions of an experiment are seldom possible with a large group. While there are in many instances matters of much greater consequence than the number of *S*s employed, there are experimental conditions peculiarly likely to be met in eccentric ways.

Then there is still another difficulty with the method of completed practice which is on quite a different level. It is a legitimate question as to what effect alcohol or loss of sleep or irrelevant sound will have upon a given performance while that performance is still in a formative stage. In other words, if our observations were restricted to perfected habits, we should learn the susceptibility of such habits to certain influences without necessarily learning the susceptibility to the same influences of that large array of performances still manifesting practice effects.

In the conduct of investigation the method of completed practice is generally utilized in an approximate form. Enough practice is given to bring the performance in question out beyond that early period during which practice effects are most pronounced. Some additional method is then likely to be introduced to care for such practice effects as are still occurring.

II. METHODS OF UNCOMPLETED PRACTICE

(*A*) *Method of Predicted Learning.* There are occasions when, even though practice cannot for one reason or another, be continued until it has become an ineffective variable, we can survey the effects of practice over a long enough period to enable us to predict their future nature. Practice curves typically show a negative acceleration beyond which their rise is extremely gradual and fairly close to linear. If we can have a given performance repeated until it is in that stage of gradual and linear improvement, practice effects can be predicted.

There is the theoretical possibility of subtracting the influence of practice order even when the performance is still improving at a rapid rate. But this possibility, which rests upon the mathematical extrapolation of efficiency values, is certainly no more than theoretical. The variability of most practice curves is such that there is almost sure to be a better procedure than that of mathematical extrapolation. In other words, if a performance is still undergoing rapid improvement due to practice the careful investigator will postpone the insertion of his experimental differential or else take care of the factor of practice order by one of the experimental procedures yet to be described.

There is one circumstance in which the use of the method of predicted learning is exceedingly safe even though that learning is still rapid. If the experimental condition consistently works against the direction of the practice effect we may be sure that that condition is effective and we may be sure of the direction of its effectiveness. If dart throwing increases in efficiency regularly and rapidly through twenty successive days of practice and if, coincidently with the introduction of alcohol or distracting noise, efficiency on later days is at a markedly reduced level, then there need be no great doubt that the experimental condition has had a detrimental influence. In this situation the fact that a performance is still improving with practice is not an important methodological disadvantage, unless one is seeking to determine something more precise than the direction of an influence. From one point of view the sharper the rise predicted from future practice, the more reliable the test of an experimental condition. If alcohol, when administered at the time of later trials, is associated with a drop in efficiency, the sharper the rise expected without alcohol, the more confidence we may have in the general direction of our results. If, on the other hand, the experimental condition should have a generally favorable influence, then the greater the expected effects of practice, the more difficulty there would be in discriminating between the expected effects of practice and the influence of the favorable condition.

(*B*) *Method of the control group.* The factor of practice order can be neglected if we divide our *S*s into two groups one of which, at a given point in the practice curve, performs in the presence of the experimental condition and the other of which, at the same practice order, performs under a relatively neutral condition. This method is widely employed. The practical difficulty is that ordinarily a large number of *S*s must be used in order that we may get two groups of equal capacity for the performance under examination. Of course, the use of large numbers is a certain insurance that whatever result the experiment may yield will not be a function of the peculiarity of some individual or of some small group. But again there is the disadvantage that the employment of large numbers of *S*s usually means that they can be subjected to few repetitions of the experiment.

A further difficulty with the method of the control group is complementary to one present in the method of completed practice. In that latter method it is possible to examine the influence of a given experimental condition only upon a relatively fixed habit, because only such a habit has passed through the stage of improving with practice. Since, in the method of the control group, it is usually possible to have no more than one or a very few sittings of every *S*, we must rest content with determining the influence of our experimental condition upon a relatively unperfected habit.

(*C*) *Methods of counterbalanced order.* In case it is possible to repeat the two or more conditions of experimentation, practice effects can be cared for by arranging that, on the average, the order of the conditions is the same. Let two conditions of experimentation be *A* and *B*. If these conditions appear in the order *A*, *B*, *B*, *A*, *A*, *B*, *B*, *A*, etc., the average order of the two conditions will be the same. The validity of this procedure, which might be called that of *equal average order*, depends upon the degree to which the practice curve for the performance involved is linear or uniformly curved. While absolute linearity or uniformity of curvature is certainly rare enough in practice curves, sudden flexion, such as would introduce important error into an experiment relying upon the procedure of equal average order, is also rare.

One useful check upon the procedure of equal average order is to compare the experimental conditions at different serial sections of the whole experiment. If the combined *A*s from the first and fourth orders show a marked difference from the combined *B*s of the second and third orders, and if the same relation between *A* and *B* holds for the *A*s of the fifth and eighth orders and for the *B*s of the sixth and seventh orders, and if analogous comparisons result from subsequent sections, we need not worry further as to whether the practice effect has actually been linear or uniformly curved.

Under certain circumstances the attainment of equal average order by such a regular arrangement as that described above is undesirable. In experiments upon the effects of drugs, for example, it may be essential to prevent the *S*s from detecting or anticipating the conditions of experimentation. In this situation equal average order can be attained by some such irregular arrangement as *A*, *B*, *A*, *B*, *B*, *A*, *B*, *A*.

Titchener took up his duties at Cornell in September 1892; he became an assistant editor of the *Journal*, as the title pages of the *Journal* indicate, in October 1895.

(6) The statement is made that "Titchener's works have been translated in more foreign languages than those of any other psychologist." What Miss Adams's evidence is I do not know. Existent bibliographies of Titchener[7] and of Wundt[8] show that Titchener's works were translated into seven languages and that Wundt's were translated into eight.

(7) In order to assist her in the establishment of the thesis that Titchener "was an unqualified German," Miss Adams says that under his editorship the book reviews of the *Journal* were almost without exception concerned with German publications." Titchener was sole responsible editor of the *Journal* for five years, 1921-1925. During that period 110 books were reviewed. Of these 22 or 20% were German.

(8) Miss Adams writes of the *Journal of Religious Psychology and Education*, founded by Hall in 1904, as his second publishing venture. It was his third. He founded the *Pedagogical Seminary* in January 1891.

(9) Regarding the "birth of behaviorism," we are informed that it resulted from Watson's revolt against the teaching of James's *Principles*. Watson inveighs, in his pioneering articles on Behaviorism,[9] against the psychological method of introspection. His 'revolt' is, as he says (and he should know), against the view that psychology is the study of consciousness. "The time seems to have come," he says in his first article, "when psychology must discard all reference to consciousness." "My psychological quarrel is not with the systematic and structural psychologist alone," but it is also with the functional psychologist who has appeared during "the last fifteen years," *i.e.* since 1898. James's *Principles* was published in 1890.

(10) We are then informed that "the robustness of Watson's assertions soon won him the professorship at the Johns Hopkins." A glance in *Who's Who* would have shown Miss Adams that Watson obtained his full chair at Johns Hopkins University in 1908, five years before his assertions were robust enough to see the light of public utterance and print.

(11) Miss Adams writes that when McDougall came to Harvard, "the American Psychological Association quickly and eagerly elected him its president." McDougall has never been president of the American Psychological Association, nor has he held any office within the association with which the presidency might be confused. He was, to be sure, president of the Ameri-

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[7] W. S. Foster, A bibliography of the published writings of Edward Bradford Titchener 1889-1917, *Titchener Commemorative Volume*, 1917, 323-337. K. M. Dallenbach, Bibliography of the writings of Edward Bradford Titchener 1917-1927, this JOURNAL, 40, 1928, 121-125.

[8] E. B. Titchener and L. R. Geissler, A bibliography of the scientific writings of Wilhelm Wundt, this JOURNAL, 19, 1908, 541-556. Three supplementary lists were published in collaboration with Geissler, three with W. S. Foster and one with S. Feldman.

[9] J. B. Watson, Psychology as the behaviorist views it, *Psychol. Rev.*, 20, 1913, 158-177; Image and affection in behavior, *J. Phil. Psychol. & Sci. Meth.*, 10, 1913, 421-428.

can Society for Psychical Research in 1921 and 1922.[10] Is it possible that Miss Adams—who writes in the tone of an informed psychologist—could have mistaken these utterly unlike offices?

(12, 13) Again it is said in the pages of the *American Mercury* that "some time before his death Titchener . . . withdrew his support from the American Psychological Association. He then invited those members whose attitude and seriousness he approved to meet with him and discuss their experimental problems. These conferences grew into annual meetings." Two errors of fact appear in those lines. First, one cannot withdraw what has never been given. Titchener never gave the Association his support. He did not attend the meetings of the Association, not even the meetings which were held in 1925 on his own campus. Secondly, his withdrawal and the organization of an experimental conference are declared to have occurred "some time before his death." That seems obvious! The phrase, if it means anything, must mean within comparatively recent times before his death, say 2, 3, 5 or 10 years. The "some time" is over 23 years, as the experimental conferences to which Miss Adams refers were organized in 1904.[11]

(14) The conferences thus organized had, we are falsely informed, "no official name, no elections, and no formal papers." As a matter of general knowledge the conferences were always known as the meetings of the Experimental Psychologists,[12] and formal papers were the rule until the 11th annual meeting in 1914, and they were, on special occasions, read after that, even as late as the meeting of 1925.

(15) "The purpose of the conferences," Miss Adams explains, "was to get exact experimental work done and he [Titchener] personally saw to it that it was done. Such departments as that at Columbia were ignored as though they did not exist." The reference to Titchener is grossly exaggerated and the reference to Columbia is absolutely untrue. The department of psychology at Columbia was one of the ten departments that participated in the organization of the group, and representatives from the Columbia laboratory have, with one or two exceptions, attended every meeting.

(16) Miss Adams must, in addition to these misstatements of fact, be charged with many half-truths. Such statements as "The undergraduate at Yale, mapping out his yearly courses, can choose between the psychology of advertising or the psychology of business," "the candidate for a Ph. D. degree [at Chicago] can find congenial work in the psychology of business, the psychology of personality, or the theory of statistics," are misleading. The reader of these lines who does not know the true situation at Yale and Chicago would derive from them a false notion of the status of psychology at these two universities.

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[10] Cf. *J. Amer. Soc. Psychical Res.*, 15, 1921, 315ff.

[11] E. G. Boring, *op. cit.*, 498.

[12] Titchener always referred to it thus both in print (cf. E. B. Titchener, Experimental psychology: a retrospect, this JOURNAL, 36, 1925, 313, footnote 1) and in speech; and the proceedings that were published of the meetings of the early conferences always bore that name (cf. this JOURNAL, 18, 1907, 383-388; 19, 1908, 288; 20, 1909, 471, etc.).

These errors may be regarded by some as trivial and as irrelevant to the central theme of Miss Adams's article. But they are not trivial, and they are relevant to the author's main contentions. The accuracy of a writer is a measure of the value of his opinions.

A good deal could be said about the value of Miss Adams's interpretation of the matters which she has thus falsely and unfairly presented. I shall content myself, however, with the discussion of but one of her opinions—and that one is the central theme of her article, viz. that American psychology is on the decline.

It is of course natural and, perhaps, on the whole, salutary that there should be a 'golden age.' The faltering steps of the 'trail blazers' seem giant strides to the graduate students and to those younger psychologists who have yet to gain a perspective in their field. The task of evaluating and comparing the accomplishments of one age with those of another is one of the most difficult with which the scientific historian has to deal. It should not lightly be undertaken. Fortunately two of the 'trail blazers' of psychology in America, two of the giants from the mythical 'golden age,' have recently given us their opinions regarding the present status of psychology. They both have the right to be heard.

Titchener said in 1925,[13] "Experimental psychology came of excellent stock, . . . its growth has been, on the whole, steady and assured. The question that remains is—What, in the year of grace 1925, is its mental age? The question is one, I take it, that has attracted us all . . . It is, of course, a question that we cannot properly answer at all, because—if I may use the figure—we stand somewhere within our own perspective, and therefore cannot judge either how far we have come or how far, to attain the status of the adult, we have yet to go. . . . We have, I think without question, passed from infancy into childhood. If . . . the intellect is most alert and most capable at the age of sixteen, we may congratulate ourselves that experimental psychology is nearing that critical point, and may expect far better things from it in the near future than have been accomplished in the past."

Cattell said in 1927,[14] "With the publication of James's *Principles of Psychology* in 1890, the opening of the laboratories at Harvard, Yale and Cornell in 1892, and the establishment of the American Psychological Association in the same year, the earlier period of psychology in American may be closed. The few survivors may look back upon it as the golden age of our science, but that is doubtless due only to the presbyopia that obscures the vision of objects near at hand. In the thirty-five years that have since passed, the number of our workers in psychology has increased to an extent perhaps without parallel in any other country or in any other science."

These masters of Psychology speak of its *rise*. Miss Adams imagines its "decline."

This note is obviously out of place in a scientific journal. It not only takes space that should be devoted to other matters, but it also fails in its main objective, since few of the readers of Miss Adams's article in the *American Mercury* will ever see or learn of this correction. The false idea that the readers of that magazine may have gained of the present status of psychology will therefore persist unchallenged. This note should have been published in the *American Mercury;* but that avenue of publication was closed against it. Its editor wrote as follows, in reply to a letter asking him whether he would, in view of the great number of factual errors occurring in Miss Adams's article, publish a correction.

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[13] E. B. Titchener, Experimental psychology: a retrospect, this JOURNAL, 36, 1925, 322 f.

[14] J. McK. Cattell, Early psychological laboratories, *Science*, 67, 1928, 547 f.

"A number of criticisms of Doctor Adams' article have been received, and I have sent several of them to her. She reports that, save on a few minor points, they are in conflict with one another. Thus I fear that it would be unwise for the *American Mercury* to go into the controversy."

Miss Adams replied as follows to a critical letter that I wrote to her. "Last week Mr. Mencken received long protests about my article from Profs. Ferberger [Fernberger] and Boring which contradicted each other so thoroughly that after they were carefully compared the glowing errors which they claimed they were pointing out were no where to be found. I read your letter with interest, hoping it would clear up the confusion, but was greatly disappointed to find that it only added to it....The only point on which you three are agreed is that William McDougall has never been president of the American Psychological Association, but there is such a difference of opinion in the letters about his relations with this august body that I feel I should wait for McDougall's personal denial before I admit even that an error. The only thing that seems certain from the controversy is that we psychologists are woefully ignorant about our own subject and find it difficult to distinguish an opinion from a fact."

Since Miss Adams named the other critics of her article, I took the opportunity to test further her accuracy of report by direct reference to the "protests" of Professor Fernberger and Professor Boring. A careful comparison of their "protests" with each other and with my own reveals absolutely no disagreement of fact.

The surprising thing about all this is, not that Miss Adams should have found factual disagreement where none exists, but that she should have been made the judge of the criticisms of her article, and that the editor of the *American Mercury*, who states (December 1928, p. 407) that his journal "simply aims to give...accurate and realistic news of what is going on in the country," should have accepted her report as final.

KARL M. DALLENBACH

DR. YOUNG ON TRAINED OBSERVERS

P. T. Young, in a problem reported under the title, "Studies in Affective Psychology,"[1] repeated a part of a problem previously reported by me upon practically the same subject.[2] In his work Young used one of the *Os* who had served in my problem and other *Os* who had not served in other problems on this subject and whose training differed materially from that of the 'trained' *O*. Young concludes, as I understand him, that the results of both of these problems depended upon the training of the *Os* and such training he holds to be prejudicial rather than effective in discovering the facts. Young says in part, "A 'trained' *O* is not necessarily one practiced in psychological observation. The important factor in training is the psychological education or indoctrination to which the *O* has been exposed. The terms of description which an *O* has acquired, the psychological facts he knows, and his theoretical views very largely determine the type of report."

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[1] This JOURNAL, 38, 1927, 157-193.

[2] J. P. Nafe, An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.

I agree with Young that previous training is important in determining the *type* of report to be expected but I do not agree that it is even a factor in determining *the report*, that is, the facts reported. He points to the methods of other sciences and I am willing to put my answer in similar terms.

In microscopy we do not turn an instrument over to one who is naïve in regard to either the use of the instrument or the nature of what one is expected to find. But no one will be found who maintains that the results obtained by one skilled in the use of the microscope and familiar with his subject matter are not as trustworthy as those of the naïve student. It must be evident that in any ordinary problem in the physical or biological sciences the *O* who knows what kind of phenomena he is looking for is at an advantage rather than at any disadvantage and his results are more, rather than less, valuable.

Young suggests the superior value of such responses as "I don't like it" which, he says, is the type of response to be expected from a naïve *O* under the conditions common to both problems. What is the answer? Obviously, if he wishes to determine whether or not the *O* likes the experience the answer has a value but if he is asking for a report upon the psychological experience, *as stated in both his and my instructions*, such an answer is valueless.

Suppose the *O* using the microscope, at the conclusion of his effort, gives the information, as a naïve *O* might, "I don't like it." Such an answer could have no importance for one waiting for a description of the matter under the glass.

Of what greater value is such a reply to one investigating the 'content of consciousness?' None. The *O* is instructed to report the experience. Some *Os* know enough to do so and to omit casual remarks and such knowledge depends upon training. The reports of such *Os* are not, it seems to me, to be considered invalid because prejudiced.

Clark University JOHN P. NAFE

MR. HOVEY ON DISTRACTION

In a report on "Effects of general distraction on the higher thought processes,"[1] Mr. Hovey states "that distraction is not a serious handicap to the group as a whole during 19 min. of concentrated thinking." Upon studying the figures presented I am unable to find substantiation for this conclusion and, indeed, consider that the opposite condition is indicated. Here is the situation and the argument. The 'experiment-group' works Army Alpha problems under conditions of relative quietness. About one and one half months later this group again works on Army Alpha problems (another form), this time under conditions of great noise. There is reported a gain of 6.4 points in the case of the second condition. This looks as if the noise condition actually helped efficiency. The gain, however, might be due to practice on the first form. The investigator wisely conducted a control experiment with a 'control-group.' This group worked on two occasions with the same materials, but a condition of quietness obtained on each occasion. A gain of 10.1 points was found in the case of the second work-period. This gain was, then, due to maturation and practice effects. Now for the argument. If the experiment-group had shown a gain of more than 10.1 points there would have been evidence of increase in efficiency due to the noise, over and above practice and maturation. If the gain in the case of the experiment-group had been just 10.1 points then there would have been evidence of 'just not any detrimental influence of the noise.' If the gain for the experiment-group had been less than 10.1 (as was actually the case), the evidence would be that the noise actually played a detrimental rôle, although it could not eliminate the practice effect.

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[1] This JOURNAL, 40, 1928, 588.

Incidentally, one may wonder when experimenters will cease speaking of 'distractors.' Why not speak simply of 'sense stimuli' until it has been proven that they are really distractors. In which case the article would be headed, "Effects of sense-stimuli on the higher thought processes."

College of the City of Detroit E. B. Skaggs

Research Interests of American Psychologists

Early in September, the Program Committee of the American Psychological Association sent a list of topics to the 700-odd members and associates of the Association as suggestions for Round Table Discussions at the 1928 meeting. The list contained 36 topics—space was left for the addition of other topics that might be desired. The members and associates were asked to check all the topics in which they were interested and in the discussion of which they would participate. A total of 212 replies was received. Individuals checked from 1 to 20 topics. In all, 1019 topics were checked. Only 22 topics were added to those given in the lists.

The number of times the different topics were checked is, in order of preference, as follows: Personality, 70; First Course in Psychology, 55; Mental Tests, 52; Motivation, 49; Childhood and Adolescence, 46; Feeling and Emotion, 44; Abnormal Psychology, 41; Individual Differences, 37; Methods of Learning, 36; Requirements for the Ph.D. Degree, 30; Facial and Postural Expression of Emotion, 30; Consciousness and Behavior, 29; Relation of Nature and Nurture, 28; Motor Learning, 28; Reflexes and Instinct, 27; Psychophysical Measurement Methods, 23; Statistical Methods, 22; Psychogalvanic Reflex Experiment, 22; Thought, 21; Relation of Anthropology and Psychology, 20; Sleep, Dreams, Hypnosis, 20; Animal Ideation, 20; Introspection as Method, 19; Mind and Body, 19; Space Perception, 19; Nature of Attention, 19; The Correlation Methods, 18; Vision, 18; Language, 18; Educational Tests, 18; Animal Behavior, 17; Esthetics, 16; Audition, 15; Seen Movement, 11; and Taste and Smell, 6.

The results indicate a strong technological interest among American psychologists. It is true that the problems of Personality (1st) and of Motivation (5th) have a theoretical as well as an applied interest; but the individuals checking these two topics more frequently checked other applied than other theoretical and experimental topics. It is also of interest that the topic the First Course in Psychology should stand second in the list, indicating a widespread desire for standardization in this direction. Of the experimental problems, those concerned with emotion show outstanding interest—Feeling and Emotion standing 7th, Facial and Postural Expression of Emotion standing 12th, and the Psychogalvanic Reflex Experiment standing 19th. Problems of statistical methodology fall in the middle of the list—the Psychophysical Measurement Methods ranking 17th, Statistical Methods ranking 18th, and the Correlation Methods ranking 29th. The problems of Sensation and Perception are to be found at the end of the lists along with Language, Esthetics, and Educational Tests.

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ERRATA

A number of printer's errors occurred in the review section of the October number of the JOURNAL. Most of them fortunately will be immediately recognized; but one occurred in the review of East's *Heredity and Human Affairs* (p. 646, sixth line of the review) which entirely changed the meaning of the sentence and which therefore must be corrected. Copy read, "The psychologist can only wish that these data had been somewhat more representative and up to date;" but the compositor set it, "The psychologist can hardly wish," etc. The change of "only" into "hardly" is curious, for the copy was perfectly clear.

J. P.

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SEX DIFFERENCE IN THE ASSOCIATION OF IDEAS[1]

By Catharine Cox Miles and Lewis M. Terman, Stanford University

Bases of the Psychological Problem

Obvious differences between the sexes expressed in behavior and not readily traceable sociologically have been attributed to basic anatomical, physiological, and psychological origins. Differentiation between groups representing the sexes can usually be made on the basis even of fragmentary data in any of these three divisions of human development, and such differentiation may have a fair degree of accuracy. Even the individual members of sex-groups may generally be assigned to their proper classification by such means, though with a certain margin of error, which will usually be largest for the psychological, less great for the general physiological and anatomical features, but not insignificant even in the case of those indexes of sex development called sex characters, whether secondary or primary. Even in that one aspect of development, the endocrine, which lies at the base of sex structure and function, no complete dichotomy has been demonstrated; even here no absolute line of demarcation can be drawn.[2] Mixed or androgynous types may, it seems, appear even with respect to gonadal secretion, so that gonads of both sexes may be present in the same individual, or interstitial

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*Accepted for publication October 20, 1928.

[1] Part of a study on sex-differences in non-intellectual mental traits, made possible through a grant from the Committee on Sex Research of the National Research Council.

[2] E. Sharpey-Schäfer, *The Endocrine Organs*, II, 1926, chaps. 53, 54, 55.

cells producing autocoids of one sex may be perversely produced in the organ characteristic of the other. Within human beings such extreme *Zwischenstufen* are rare, but they are known to exist, and they emphasize by their presence the range of distribution of sex characters in the human species, from the normal masculine to the normal feminine. Because the endocrine basis of sex has been demonstrated, a basis definitely prepotent during its period of activity, and because its effect upon structure and function in general has been demonstrated, there is a tendency to attribute sex differentiation in general to this origin. Furthermore, since a genuine and obviously fundamental difference has been demonstrated in this regard, and since other and secondary aspects of development appear to be conditioned by it, an inclination naturally arises (a) to look for further and often more remote differences that might prove as essentially diagnostic, whether as primary or secondary features, because of their being definitely the result of the demonstrated endocrine differentiators; and (b) to look for further and other differences as essentially diagnostic which may be due to some now obscure, but equally potent, differential causes.

Interest at present does not lie in demonstrating a superiority of one sex with corresponding inferiority of the other; for instance, current writers are not trying to prove an infantile type of woman or a primitive savage type of man, or again a feeblemindedness or mental weakness of woman and a brutal and tyrannical strength of man. Rather, the concern is with an evaluation of the available, experimentally demonstrated findings which may contribute to an understanding of present development under existing conditions,[3] and may also in part suggest the possibilities of future development under modified conditions. The problem is peculiarly complex because social conditions, the weight of tradition and the pillars of custom, maintain an equilibrium now that is not necessarily according to Nature's original construction plan. What we are able at best to ascertain is a series of sex differences as they appear under the present scheme of things; with perhaps some comprehension of the degree to which they are essential or fundamental as we see their deviations or fluctuations under more or less systematically modified conditions.

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[3] H. C. Lehman and P. A. Witty, Play interests as evidence of sex differences in aesthetic appreciation, this JOURNAL, 40, 1928, 449-458; also J. A. McGeoch, The influence of sex and age upon the ability to report, *ibid.*, 40, 1928, 458-467.

Some at present assume the existence of a masculine mind and a feminine mind,[4] and to this point of view the researches of Havelock Ellis[5] and others may appear to give support. "The masculine method of thought is massive and deliberate, while the feminine method is quick to perceive and nimble."[6] "Girls are aided by a better memory and a variety of emotional and moral qualities which involve a greater docility. Boys, however, possess other qualities which contain the promise of ultimately greater intellectual development."[7] "Men are better able to apply what they have learnt; men are very much more inclined to supplement what they have learnt by reflection or further independent investigation. . . . Women dislike the essentially intellectual process of analysis. . . . Women dislike rigid rules and principles and abstract propositions, they feel that they can do the right thing by impulse without needing to know the rule." "These characteristics, by which on the average women seem to be marked in relation to men, are *probably correlated with instinctive and emotional qualities which may fairly be regarded as organic*" (our italics). But again: "The facts are far too complex to enable us to rush hastily to a conclusion as to their significance. The facts, moreover, are so numerous that when we have ascertained the precise significance of some one fact, we cannot be sure that it is parts, and perception, thought, and rational action are dependent on the functioning of the association process. It may be concerned with 'objects,' or 'ideas,' 'things thought of,' 'the thinking of them' or even 'mental attitudes' (complex undefined background). This is a matter of emphasis and of terminology; "so far as it stands for a cause it is between processes in the brain." We may say with James, association is "a result of the laws of habit in the nervous system," or with Titchener, "whenever a sensory or imaginal process occurs in consciousness, there are likely to appear with it (of course in imaginal terms) all those sensory and imaginal processes which occurred together with it in any earlier conscious present."[10]

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[4] M. E. Haggerty and E. J. Kempf, Suppression and substitution as a factor in sex differences, this JOURNAL, 24, 1913, 414-425. "It must be said that the experimental study of adult sex differences has not as yet yielded any very significant results. There seems to be a general conviction that there is a feminine type of mind different from the masculine but in just what this difference consists we have as yet no measured experiments to prove." H. T. Moore, Further data concerning sex differences, *J. Abn. Psychol. & Soc. Psychol.*, 17, 1922, 210-214. "It is hard to escape the conviction that the original nature here depicted is of two fundamentally different sorts, and that the two types could hardly be permanently adapted to identical interests . . . There are very considerable and ineradicable differences in the original capacities of the two sexes for certain types of enthusiasm."

[5] Havelock Ellis, *Man and Woman*, 6th ed., 1926. Cf. also Joseph Jastrow, *Character and Temperament*, 1915 p. 366. "There is but one supreme natural differentiation—that of sex. If there is any distinctive group-psychology, it must be that of the group of men and the group of women. For men and women are organically different; which means that the physiological differentiation in the reproductive system involves a contrasted psychology."

[6] Ellis, *op. cit.*, 233.

[7] *Ibid.*, 261.

The difficulties in the way of mapping the course of integration or organization of stimulus material into the complex images of memory, imagination or generalization, in whatever mode, have been met and in part surmounted by psychologists whose aim was the observation and dissection of the individual observer's mental processes.[11] No study attempting group comparison has undertaken to sift the individual material for the comparison's sake. The task is clearly too formidable. Effort has been directed rather along the line of the verbal response technic, which since Galton's first study[12] has seemed to offer a kind of plummet for the ideational complexes that make up at least the upper layers of thought. The word association method plus introspection can go much further than this, but, as we have seen, the results are unwieldy for group study. In its simplest form the technic presents soundings of interests and mental occupations. For separate individuals, equally clear and regularly dependable samples of these activities are not obtainable, because (a) the soundings are from different levels, *i.e.* depending upon the degree of conscious or unconscious coöperation of the subject; (b) they are from regions in various degrees of neural tempest or calm, *i.e.* depending upon somatic conditions, fatigue, mental stress or relaxation, whether natural or induced; (c) they are variously predisposed by the character of the stimuli, so that the conditions of (a) or (b) or both may result in the case of subjects otherwise fairly equally balanced and motivated for the experiment. These confusing elements can in part be eliminated by the set of the experiment; the residuum of confusion can sometimes be stripped away. In certain instances these other factors are the very elements to be isolated and compared. In any case the experiment may be so set as to throw the emphasis either on the soundings, or on the accessory aspects, and the intrusion of other elements may be to a considerable degree diminished or eliminated.

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[10] Titchener, *op. cit.*, 378.

[11] H. J. Watt, Experimentelle Beiträge zu einer Theorie des Denkens, *Arch. f. d. ges. Psychol.*, 4, 1905, 290-436.

[12] Francis Galton, Psychometric experiments, *Brain*, 2, 1879, 149-162.

"There are marked individual differences in association," says Titchener, apropos of his contrast between the quick and the slow learner. The associations of adults are complex, of children simple. The associations of the intelligent are rich, of the feebleminded meager and threadbare. The associations of the normal balanced mind are clear cut, traceable, and when traced explicable; those of the abnormal mind are often distracted, inhibited or adverted by complexes, traceable only through intricacies of distorted meaning and reference, and in the last instance frequently inexplicable. Differences in the quantity and quality of associations may, in the case of the various groups, be obviously attributable to innate differences in the quality and strength of mind, or perhaps as obviously to training and experience. On the assumption that adequate analysis of mental associations through their symbolic expression may give a useful index to mental behavior and ability, intelligence tests and achievement tests are built. Controlled association material, and even certain free association technics, are depended upon as indexes of mental age or brightness. In the intelligence test scales no very significant sex differences in the handling of material of this sort have been found in tests constructed to measure mental development from age level to age level. In the tests of adults the case is not so clear, and in various association experiments of the free verbal type both qualitative and quantitative differences have been reported.

Classification of Word Association Data on Sex Difference

In bringing together the findings of psychological investigations of word associations where sex differences have been considered, arrangement of the material is suggested by content. The two main points of departure, in terms of the response and the responding subject, and in terms of the stimulus, might also be tend to predominate. Here, and otherwise as well, degree of understanding of the nature of psychological experimentation, practice in laboratory observation, and scientific training in general, will have their influence on response type. (It is assumed that controllable somatic conditions are maintained at a fairly constant level.)

Group studies of response type indicate typical difference trends for age, sex, cultural or educational level, and degree of mental normality. Each of these may be interpreted by comparison or in terms of the other, where each in part indicates the significance of the other comparison. A suggestion of the frequently more superficial response type of children, especially young children, is given by the Rosanoffs.[19] In this case the superficiality is due not so much to failure to coöperate in the adult sense, but rather to the imperfect attention of childhood plus (especially with the younger children) lack of familiarity with the words used as stimuli. In any attempt to explain adult responses as 'childish' both of these trends must be taken account of. The response types have been particularly observed in the normal discrete experiments of Jung and Riklin,[20] Fürst,[21] and Hans Huber,[22] and in the distraction experiments of Jung and Riklin. The latter have proved illuminating where tendencies in the former were too slight to be more than suggestive.

In the normal experiment the findings of the three investigations (whose chief emphasis is on other matter) present experimental facts on sex differences; and these in the main corroborate one another. The distraction experiment of Jung brings out more clearly the trends, noted in the normal experiment, that are produced by conscious or unconscious failure to coöperate with the intent of the experiment: trends important in reference to sex differences. Jung's normal experiment was one in which the *Ss*, 22 women and 16 men, responded by giving orally the first word they thought of, to each of a series of 200 words presented as auditory stimuli. The reactions were timed with a one-fifth second stop-watch held by *E*, who also wrote down the responses. Two men *Es* (Drs. Jung and Riklin), whose reactions were found to be comparable, were in charge. The *Ss* were selected with a view to their being average normal individuals, and probably they were justly rated as such. The 200 stimulus words were selected from a list of 400, which included 231 nouns, 69 adjectives, 82 verbs, and 18 adverbs and numerals. The composition of the

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[19] I. R. Rosanoff and A. J. Rosanoff, A study of association in children, *Psychol. Rev.*, 20, 1913, 43-89.

[20] C. G. Jung and Fr. Riklin, The associations of normal subjects, in Jung's *Studies in Word-Association*, 1919, chap. 2, 8-173.

[21] Emma Fürst, Statistical investigations on word-associations and on familial agreement in reaction type among uneducated persons, *ibid.*, chap. 11, 407-446.

[22] Hans Huber, Ueber den Einfluss von optischem oder akustischem Reiz und grammatikalischer Form des Reizwortes auf den Assoziationsvorgang, *J. f. Psychol. u. Neurol.*, 23, 1918, 171-207. An error in the labelling of Huber's tables transposes all of the headings, apparently consistently. For J read AM; for AM read J throughout Huber's appended tables.

Jung list is too well known to require further description.[23] The words were selected as far as possible from those in common use. No attention was paid to the number of syllables.

The first distraction experiment made use of the third hundred of the 400 stimulus words. To these, reactions were made during a condition of inner distraction. The *Ss* (the educated group only) were requested to attend as far as possible to the 'A-Phenomenon,' *i.e.* the "sum of those psychological phenomena directly evoked by the perception of the acoustic stimulus." Directly after each reaction they described what they had noted during the experiment. The second distraction experiment used the fourth hundred of the word list, to which reaction was made under a condition of outer distraction. The competing stimulus was arranged as follows. To the beat of a metronome the *S* had to make pencil marks about 1 cm. in length. The beats for the first 50 reactions were 60 per min., for the second 50 reactions, 100 per min.

TABLE I

JUNG'S COMPARISON OF THE SEXES IN A NORMAL WORD ASSOCIATION EXPERIMENT*

(Results expressed in percentages)

Class of response	Men	Women
A. Inner Associations	38.6	40.7
B. Outer Associations	54.0	53.6
C. Clang Reactions	2.0	2.0
D. Residual group	3.9	2.4

*Abbreviated from Jung's table.

In the normal experiment Jung found, in comparing averages, minimal differences between the sexes. "With few exceptions the figures are in substantial agreement; at any rate, there is an absence of the distinct difference which, *e.g.* separates the group of the educated from the uneducated." In the distraction experiment larger differences appear, indicating in the main a lesser distractability of the women. The slight differences that did occur in the averages for the normal experiment are given in Table I. Calculations of the probable correctness of these differences between the entire groups of men and women (Table I) indicate, as Jung has stated, that they are not significant.

Considering averages for subgroups of *Ss*, Jung finds that both men and women of the educated group show smaller percentages of inner associations, and larger of clang reactions and residual responses, than the general averages for the combined groups. The uneducated show the opposite tendency. A possible difference in reaction type between men and women as groups is barely suggested. If present, it consists in the relation and balance of inner associations and the three other categories of response. Individuals who attend most seristrokes to the beat of a metronome while carrying on an association test at the same time may for some of them be less of a novelty than for many of the men. This experience would be quite as much that of the educated women as of the uneducated.

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[23] The complete list used by Jung and Riklin in this experiment is not given by them. Parts of it appear in Jung and Riklin, *op. cit.* 135 ff; 356 ff. In the translator's preface to the same work Eder gives a list of 100 words modified in form for use in English: *op. cit.* Transl's Preface vii. See also C. G. Jung, The association method, this JOURNAL, 21, 1910, 219 f.; and S. C. Kohs, The association method in its relation to the complex and complex indicators, *ibid.*, 25, 1914, 593.

TABLE III

COMPARISON OF RANGE FOR THE TWO SEX GROUPS IN JUNG'S OUTER DISTRACTION EXPERIMENT

Compared Groups	First Experiment Inner	Outer	Clang	Residual	Second Experiment Inner	Outer	Clang	Residual
Per cent of men scoring above the 50th percentile of the women	23	69	54	54	15	77	23	95
Per cent of women scoring above the 50th percentile of the men	69	19	25	38	63	38	50	6

TABLE IV

PROBABLE CORRECTNESS OF THE DIFFERENCES BETWEEN THE AVERAGE PERCENTAGES FOR THE SEX GROUPS IN JUNG'S OUTER DISTRACTION EXPERIMENT

	First Experiment				Second Experiment			
Kind of Association	Men Ave.	Women Ave.	Diff.	P.C.	Men Ave.	Women Ave.	Diff.	P.C
Inner	29.9	37.2	7.3	0.96	25.8	32.9	7.1	0.96
Outer	57.5	51.5	−6.0	0.92	62.6	57.9	−4.7	0.84
Clang	6.0	6.7	0.7	0.61	4.1	6.4	2.3	0.81
Residual	6.0	4.7	−1.3	0.80	7.8	3.7	−4.1	0.99

Summarizing, we find from Jung's results, as far as response type is concerned, that in a normal word association test very small but probably significant differences between the sexes are present, and that in a distraction experiment the same differences are present, but to a more pronounced degree. The differences consist in a certain relation between inner (meaningful) and other (outer, clang and residual) reactions, and this appears to indicate the extent of deliberate coöperation rendered to the experiment. The women are more coöperative than the men in this respect, and show the tendency more pronouncedly the greater the distraction offered. But other subdivisions, namely, the cultural, show far greater differences than the sex groups in this regard. We may say then that women tend to be like the uneducated in their response type.

A summary of results for sex differences is incomplete without their interpretation in terms of the cultural, educational, or occupational status of the groups. Among the educated, where the difference between the response type of the sexes is least, the educational difference is also least. Among the uneducated, where the difference is greater, the difference in education is also distinctly greater and in the expected direction. Thus when the men show a more superficial reaction type than the women, the difference cannot at once be assumed to be due to sex, as the same difference appears to a greater degree between the educated and the uneducated and appears to be measured by the cultural status. The educated "exhibit a distraction phenomenon as compared with the uneducated," and "we regard it as proved that the difference between the educated and uneducated reaction types . . . is entirely functional and has only the significance of a phenomenon of attention." As the same contrast in somewhat less degree appears between the sexes, and as the latter differ also in cultural level, the same phenomenon of attention and functional difference in type of reaction may be further concluded.

Had Jung based his conclusions on as painstaking a study of 500 *S*s whose education and experience were similar or measurably dissimilar, in place of the 38 persons selected at random from two unequal social levels, his findings would doubtless offer the last word on the subject. But under the circumstances he brings us to the conclusion that the matter is still open to question and that the sex differences demonstrated are perhaps completely accounted for in terms of education and culture.

TABLE V

COMPARISON OF THE ASSOCIATION RESULTS OF FÜRST AND JUNG WHO USED THE SAME METHODS

(Table from Fürst)

Kind of Association	Men		Women	
	Fürst	Jung	Fürst	Jung
Inner	76.9	40.5	78.9	46.6
Outer	21.0	55.6	19.7	49.4
Clang	0.1	0.6	0.2	0.7
Residual	0.1	2.4	0.7	2.3

If we should interpret Jung's results in terms of children's reactions (Rosanoff),[14] the inattention of the men produces a response tendency similar to that of youngsters; the women on the other hand show more of that unfamilarity with the stimulus noted in the most youthful responses. However, we do not believe that the situations are more than superficially comparable, and we can not call either adult type 'childish' because of a slight factitious similarity in one aspect.

In a study of 42 men and 58 women drawn from 24 families, Fürst,[15] who follows Jung in method and treatment, notes the sex differences in response and gives the averages which illustrate them. As in Jung's study, the point stands out here "that education is one of the factors which most seriously influences the nature of the reactions." Fürst's *S*s are from a lower cultural level and of less education and intellectual development than were Jung's. When her cases are compared with Jung's (Table V), very large and significant differences appear, although variations in method seem to be ruled out by the close collaboration of the two investigators. Such large differences may be connected with the large cultural differences present. The question of the suitability of

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[14] I. R. Rosanoff and A. J. Rosanoff, *op. cit.*, 46-50, 88.

[15] Fürst, *op. cit.*, 407-446.

The light thrown on reaction type (a) by complexes (Table IX), and (b) by disturbances in memory (Table X), seems to add a small increment to the evidence for a consistent sex difference. The women in Huber's study reveal more complexes than the men, and they show less disturbance in recall; in other words, they are serious enough in general attitude so that emotionally colored stimuli break into the guarded complex region of personal experience in the original experiment; and they are also more successful in recall. These behavior indications, combined with longer time requirements, fewer clang and other superficial responses, especially a tendency away from the simpler grammatical associations to the more difficult and meaningful ones, appear to indicate that the group of women as in Jung's normal experiment is more subjective and coöperative than the men in fulfilling the stated requirements of the experiment. Huber adds no distraction experiment to emphasize this apparent tendency.

TABLE IX

Huber's Results for Complexes

		Jung words		A & M words	
S	No.	Visual	Auditory	Visual	Auditory
Men	14	7.1	8.1	4.3	6.4
Women	17	12.5	9.9	5.2	8.4

TABLE X

Huber's Results for Memory Disturbances in Word Association Experiments

		Jung words		A & M words	
S	No.	Visual	Auditory	Visual	Auditory
Men	14	22.6	27.6	20.2	16.5
Women	17	20.1	27.7	15.2	14.2

We conclude that Huber's data for normal *S*s show small and probably insignificant sex differences in the normal experiment using Jung words. Complexes and disturbances in reproduction appear to substantiate the view that the women are more conscientious in response and that they keep closer, through meaningful reaction, to the hidden 'complex region,' and so tend to make attacks on their 'guard' more often than the men. It is not impossible that the sex of the *E* is entirely accountable for this phenomenon.

Hull and Lugoff,[17] for a list of 100 Jung stimulus words, find about the same number of complex indicators for the two sexes, 80 for the men, 76 for the women. Most of the words tend to indicate about equally for the sexes, but a few yield quite different values: *e.g.* for the women, *fall* and *choosing* are more diagnostic; for the men, *caring for*, *despise*, *pride*. The 100 *S*s averaged slightly over 66 complex signs each, their scores being 3,319 for the men and 3,320 for the women, indicating no difference in total number.

Differences were shown in kind of indicator, however, for of the four important indicators, (a) repetition of stimulus word, (b) assimilation of stimulus word, (c) long reaction-time, (d) defective recall in repetition of reactions, the women show the larger number of signs in all categories except the last, *i.e.* in all except the memory test. In the four unimportant indicators, (a) no response, (b) repeated use of the same response word, (c) strange or senseless reactions, (d) obvious perseveration of idea, the women show a preponderance in the first two, the men in the last two.

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[17] C. L. Hull and L. S. Lugoff, Complex signs in diagnostic free association, *J. Exper. Psychol.*, 4, 1921, 111-136.

The excess of the women in reaction-time the writers do not attribute, as do Wells, Jung and others, to the fact that *E* was a man, for if this were the case, they say, the women would for the same reason show an excess in the other complex signs as well, which does not occur. The women show an almost equal excess in the repeated use of the same response word and this is negatively correlated with long reaction-time. Conscientious coöperation is thought to be the cause for both long reaction-time and absence of defects in recall. Repetition of stimulus word also accounts frequently for long reaction-times in the women.

It should be especially noted that for three of the four signs which the authors believe to be really diagnostic, women yield the largest number of complex indicators. For two of the four unimportant indicators, they also yield the largest number. The remaining two total 0.67 in all, or less than 1% of all the signs, a very insignificant amount. Defective recall, the largest indicator for the men, does not occur in the original association experiment. It appears in a controlled association test in which memory rather than free association is the most important function. As the women have shown themselves more conscientiously coöperative from the first, we might expect more accurate performances in the reproduction test. Thus it appears that all of the 'complex signs' in the original experiment may be influenced by the sex of *E*, even if all do not correlate positively with one another. The matter is still open to question until the investigation is repeated under similar conditions by a woman *E*.

Burt and Moore[28] state that "the course of ideas is . . . more frequently disturbed in women by the various signs of complexes." It is their contention that "when words relating to specific emotions are distinguished and the times are calculated separately, the emotional influence and the sex differences are more prominent." They give us no specific data to support these statements.

Herrington and Willits's[29] study shows reaction-time as a measure of complexes in various categories, differentiating the sexes. Using men *E*s for the men, women for the women, they discovered more slow reactions among the women (48% compared with 40% for the men). The word list was highly selected, containing 30 words, mainly of high tension character for one sex or the other, or for both.

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[28] C. Burt and R. C. Moore, The mental differences between the sexes, *J. Exper. Pedag.*, 1, 1912, 273-284, 355-389.

[29] L. P. Herrington and J. M. Willits, A study of sex differences in interest trends as indicated by the free association method. An unpublished experimental study made in 1927 under the direction of Professor P. R. Farnsworth, Stanford University.

accidental momentary attitude, but corresponds to an important psychological characteristic—one which is maintained amid altered conditions." Jung says definitely that all women are not necessarily of the predicate type, neither are all men of the objective type. Many uneducated women persistently maintain their response type under distraction without showing the 'predicate' characteristics, while there are men of both groups who are definitely subjective. The educated women show relatively more true predicate tendencies than any other group. Here we are reminded (although Jung makes no mention of it) of the personal and value emphasis in their occupation and training and also of their literary (interpretative) rather than scientific (factual, experimental) training.

Among Fürst's *S*s the predicate type was present in both sexes, but relatively more women than men appeared to belong to this class. Relatively more men than women maintained an objective intellectual attitude. The predicate type seemed to increase in numbers with advancing age, but any noticeable change in this regard appeared later in life with men than with women.

In a table of comparison[22] between the sexes which is given as the basis for many of the conclusions on sex differences, we find the differences in the number of 'value predicates' (subjective type) and of 'definitions' (objective type) outstanding. It is these differences plus other slighter ones that lead Fürst to conclude that "the female subjects have a subjective 'personal' attitude whilst the men have generally an objective intellectual attitude." We have derived the PEs and the probable correctness of the difference for the value predicates computed from the individual tables given, and find them to be for the women, average 24.7 PE ± 3.2, for the men, average 13.6 PE ± 1.6. The difference is 11.1, and its probable correctness is 0.98, or practical certainty.

The original figures for 'definitions,' in which the men exceed the women 10 to 1 according to the table, are not available. Inspection suggests that these would show even higher probable correctness than the predicates.

A subjective personality type appears on the basis of these findings to be indicated more often among the women than among the men, and to be especially found among the cultured women. But Jung says that we may not assume it to be a sex difference because it is not common to women or absent from men. The best evidence for its existence is in the persistence of meaningful and subjective responses under distraction. But may we not attribute this, as we have said, in part at least to training and interest? Although there can be no question that a difference has been demonstrated for these sex groups, the question of its meaning will not be settled until comparable *S* groups are examined.

(*C*) *Interest type:* (*1*) *Extent of interest.* A series of studies of the community of ideas as evidenced by continuous word associations appears to show that men have larger surface vocabularies than women.

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[22] Fürst, *op. cit.*, 442.

Jastrow,[33] Calkins,[34] and Manchester[35] have contributed results, the combined evidence of which is important on this problem. Each of the three experiments was carried out along similar lines; Calkins and Manchester having followed Jastrow's procedure. The *Ss* were all university students, first, 25 men and 25 women at the University of Wisconsin, (Jastrow, 1891); second, 25 women at Wellesley (Calkins, 1896); and third, 3 groups of 25 men each and 3 groups of 25 women each at the University of California (Manchester, 1905). The procedure consisted in asking the *Ss* each to write a list of 100 words (1) "at their leisure," and (2) "as rapidly as possible." The results gave information (a) on the average number (percent) of different words used by groups of 25 for each of the sexes, (b) on the number of unique words used by the groups, and (c) on the character of the words used. Conclusions to date on the first two points are based primarily upon these three researches. With reference to the third point, valuable results are available from other studies as well, and these we have attempted in later pages to bring into line as far as they concern sex differences. Figures from Manchester's table presenting the results for the three studies, to which we have added the percentages for the 'unique words,' give the various data in comparable form for both unique (unrepeated) and vocabulary (different) words.

TABLE XI

SEX DIFFERENCES IN COMMUNITY OF IDEAS AS INDICATED BY CONTINUOUS WORD ASSOCIATIONS

Investigation	Unique or Unrepeated Words*				Vocabulary or Different Words†			
	Number		Percent		Number		Percent	
	Men	Women	Men	Women	Men	Women	Men	Women
Wisconsin	746	520	29.8	20.8	1376	1123	55.0	44.9
Wellesley		868		34.7		1306		52.2
California Set I	1000	949	40.0	37.9	1471	1397	58.0	55.9
California Set II	1170	1079	46.8	43.2	1583	1509	63.3	60.4
California Set III	1079	978	43.2	39.1	1489	1407	59.6	56.3
Combined Calif. Set	1975	1950	26.3	26.1	3119	3048	41.6	40.6

*A word used but once and by but one person.

†Different words may be used more than once but each is counted as one only.

In both percentage of unique words and percentage of different words the men exceed the women in every pair of groups (see Table XI). The Wisconsin *Ss* show a large difference in number of unique words as compared with any other groups of 25. Manchester suggests that this may be due to different methods of counting singulars and plurals: as the same, or as different words. In the California study they were counted as two different words. If Jastrow did not do this, it would account for his lower percentage of unique words. Another possible explanation is implied in Jastrow's statement, quoted by Calkins, that he would have rejected a list containing a long series of prepositions as too artificial and purely verbal. "What we want . . . is one hundred different ideas." Such a systematic weeding out of lists in a single group would of course give the resultant percentages a different significance from those obtained from unselected lists.

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[33] Joseph Jastrow, A statistical study of memory and association, *Educ. Rev.*, 2, 1891, 442; A study in mental statistics, *New Rev.*, 5, 1891, 559-569; Community and association of ideas; a statistical study, *Psychol. Rev.*, 1, 1894, 152-158; Community of ideas of men and women, *ibid.*, 3, 1896, 68-69.

[34] M. W. Calkins, Community of ideas of men and women, *Psychol. Rev.*, 3, 1896, 426-430. Cf. also Cordelia Nevers, Dr. Jastrow on community of ideas in men and women, *ibid.*, 2, 1895, 361-367; Amy Tanner, The community of ideas of men and women, *ibid.*, 3, 1896, 548-550.

[35] G. S. Manchester, Experiments on the unreflective ideas of men and women, *ibid.*, 2, 1895, 50-66.

As noteworthy perhaps as the regularly larger percentages for the men is the decrease in the percentage differences between the sexes when groups of 25 are combined into larger groups (California combined set of 75). This suggests that the differences noted in the smaller groups, probably sufficiently reliable as measures in these particular cases, may not hold generally when other groups and especially large groups are examined.

In two other experiments, presumably with university students, Jastrow finds that when 10 common words are given one at a time, to each of which 5 responses are made, the number of repeated words, *i.e.* the community of ideas again, is (a) equal for the sexes (1891), and (b) larger for the men (1894). In a similar semi-discrete association experiment with high school boys and girls (1891) the girls were found to use distinctly fewer different words than the boys. In these three experiments the number of individuals is different from that in the studies first reported; the percentages are therefore not comparable with these and we do not give them. They are, however, comparable as between the sexes, experiment for experiment, and the conclusions are perhaps of interest as supplementary data.

Thompson,[36] Burt and Moore,[37] and Edwin Huber[38] in tests of a like character, *i.e.* semi-discrete word associations, oral stimulus, written response, found similarly that women tended to show rather less community of ideas than men. Thompson gave university students, 25 men and 25 women, a series of 10 words in response to each of which they were to write down as rapidly as possible for a fixed time, 1½ min., the train of ideas it started. The 10 words were selected from the common interests of university students. Of the 10 words, one called out more numerous associations from the men; for 4 the responses were equal, for 5 the associations from the women were more numerous. Combining results, it appeared that the men were more apt to stick to one topic, the women to cover a number and to have a greater number of associations. This agrees with Jastrow's results for discrete association series: *i.e.* when the stimulus word is given the women show less community of ideas (more variety) than the men. The results, however, are based on too few cases to be conclusive.

Burt and Moore report an experiment in which, proceeding from a given stimulus word, the *S*s gave orally their associations, in a continuous train. The women were found to alter their themes and topics far more frequently than the men, while the men showed a greater variety of associative connections between one idea and another with the same general theme. This seems to indicate results like those of Thompson but no figures are given to show the degree or significance of the differences summarized. Burt and Moore also report a second experiment in which responses in a discrete association test are examined. They give no data, but state that there are "conspicuous sex-differences" in the nature of the replies in children from five to eleven "even at the youngest stage."

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[36] H. B. Thompson, *The Mental Traits of Sex*, 1903, 100-109.

[37] Burt and Moore, *op. cit.*, 382.

[38] Edwin Huber, Assoziationsversuche an Soldaten, *Zsch. f. Psychol.*, 59, 1911, 241-272.

Edwin Huber, using a list of 46 stimulus words, to which separate responses were given in writing, examines the word associations of 159 soldiers, aged 18 to 26, comparing them with Reinhold's[39] similarly obtained data from school-girls. The soldiers showed significantly fewer preferred reactions than the school-girls, *i.e.* they had less community of interest (more variety). Huber interprets this to mean that the soldiers were more guided by meaning than were the school-girls, and hence gave a more primitive type of response.

As the case stands, the evidence so far presented shows for continuous association a small but persistently greater uniqueness of response on the part of the males with greater community of ideas registered for the females. Where the groups are large the difference tends to disappear. In the discrete and semi-discrete experiments the situation is reversed; the women show less community of ideas, the men more. The matter is viewed from a somewhat different angle by *Es* who have attempted to correlate the extent of community of ideas with some other factor.

Certain experiments by German and American investigators have related the factor of age (especially mental age) to the diversity or community of ideas. Unfortunately for the present comparison, these were all carried on by the Jung type of discrete word association, in which a single response word is given to each of a series of stimulus words. Hence the findings are not comparable with those in the 'continuous series' of Jastrow, Calkins, and Manchester. They should be taken into account, however, in any study where sex differences are found in community of ideas, as possibly interpretative of such differences, and they may be compared with other results got by the discrete association technic. Meumann[40] concluded from studies of school children that "the richness in ideas and originality of reproductions, or the deviations of the reproductions of an individual from the average of the reproductions of children of the same age" (uniqueness) is a dependable measure of intelligence. Ziehen[41] found that children showed a greater number of individual associations than did adults. Wreschner's[42] results agree with Ziehen's, but he had only two children among his *Ss*, so his findings are no more than possibly corroborative. The tables of

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[39] F. Reinhold, Beiträge zur Assoziationslehre auf Grund von Massenversuchen, *ibid.*, 54, 1910, 183-214.

[40] E. Meumann, Intelligenzprüfungen an Kindern der Volksschule, *Experimentelle Pädagogik*, 1, 1905, 91.

[41] Th. Ziehen, Die Ideenassoziation des Kindes, *Abh. a. d. Geb. päd. Psychol. u. Physiol.*, 1, 1898, 1-66.

[42] A. Wreschner, Die Reproduktion und Assoziation von Vorstellungen, *Zsch. f. Psychol.*, Erg. Bd., 3, 1907, 1-599.

Thumb and Marbe[43] and of Saling[44] are based on sufficiently large numbers to be significant in their results; they show that children diverge more than do adults from the common responses.

Reinhold proceeds to test the tendency suggested, namely to discover whether a continuous trend can be found from childhood to adulthood, where community of ideas increases (*i.e.* uniqueness decreases) more or less consistently with age. A group of 300 girls, ages 7 to 17, in 10 school grades were given a series of 46 words orally, writing the responses. The ratio of preferred reactions to all reactions averaged just more in the 5 upper classes than in the 5 lower, but the differences were quite small, and did not consistently increase from class to class. Furthermore, when the school-girls of 4 classes were divided class by class into more and less intelligent groups, no agreement was found between intelligence and variety of response. The question as to whether these findings contradict Meumann's depends upon whether the two experiments were comparable. In any case they fail to support the general validity of a conclusion that uniqueness and its opposite, community of ideas, in a discrete association test are measures of schooling or mental age. Similarly, the results with three groups of persons compared by Kent and Rosanoff[45] in their discrete word association test show both children under 16 and persons of common school education to have very slightly less than the average number of unique response words, while persons of college training have slightly more such words (the women of the highly educated group had just more unique reponses than the men). Kent and Rosanoff conclude that "this comparative study does not show any considerable differences corresponding to age or sex." Even the differences noted between cultural groups they believe to be the result of chance fluctuations such as might be reversed in another group of 1000 cases.

In conclusion we may say, with respect to community of ideas as indicated by free continuous association, that women and girls have probably a somewhat more limited field of expression than men and boys. In free discrete association tests the reverse seems to be the case. It is possible that time and place alter the situation, especially in the case of the free continuous association. We are inclined to the view that with more equal training and opportunity for experience of the sexes the situation shifts, and that this shifting may account for the Wisconsin-California differences. In the discrete association test, a matter of verbal facility as well as one of conscientious effort (such as occurs in the better school grades of girls) may enter in. Perhaps this is a fundamental sex-difference; perhaps again it has to do with training, opportunity, and experience, conditioned from the pre-school age onward.

(*2*) *Character of interest.* Extent of interest compared for the sexes shows small, possibly undependable, differences, conditioned perhaps by the social situation. Character of interest shows larger differences, but these are more clearly the result of social pressures and of habit.

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[43] A. Thumb and K. Marbe, *Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung*, 1901, 17 ff.

[44] G. Saling, Assoziative Massenversuche, *Zsch. f. Psychol.*, 49, 1908, 421 ff.

[45] G. H. Kent and A. J. Rosanoff, A study of association in insanity, *Amer. J. Insanity*, 67, 1910, 36-96, 317-390.

The word association test obviously taps surface thought. It seems particularly adapted, when conducted as an unreflective response experiment, to bring to light habitual trends of mental occupation or lines of interest. It is quite probable that it does so except in two special instances, where the *S* fails to coöperate with the intent of the test, either (a) by reason of sophistication (Jung's cultured type), or (b) by reason of embarrassment (complex indication). The interference offered by these two possibilities is recognized as reacting possibly unequally for the sexes (a) because of the unequal sophistication of *S*s used, as far as reaction to this sort of experiment is concerned, and (b) because of the unequal emotional attitude induced in the two sex groups by reason of the investigator's belonging to one sex or the other.

In addition to the possibly fundamental difference referred to between results obtained for the two sexes there is, further, the formal difficulty inherent in any attempt at a classification of results. Classifications are subjective, and the results of one investigation classified by its investigator will never be strictly comparable with those that have been classified by another, even if he use with as much care as possible the same scheme of evaluation.

In the continuous association test the combined results of Jastrow, Calkins, and Manchester present a representative view of the sex differences in surface interests of university students in the 90's and in the first decade following. In the Wisconsin and California studies a group of men was in each case compared with an equal number of women. We may assume where there is a consistent preference of one sex appearing in all of these groups, or in 3 groups out of 4, for response in a given department of interest, that the difference is probably a real one. Manchester has presented these preferences in a table and we follow her, adding also the evidence of the Wellesley group. The men predominate in all 4 groups in (1) verbs, (2) implements and utensils, (3) occupations; they predominate in 3 out of 4 of the groups in (4) animal kingdom, (5) abstract terms, (6) parts of body, (7) other parts of speech, (8) miscellaneous, (9) meteorological and astronomical (10) mineral kingdom. Similarly, the women preponderate in all 4 groups in (1) wearing apparel and fabrics, (2) buildings and building materials, (3) interior furnishing, (4) educational, (5) arts, sciences, (6) amusements; and in three out of four in (7) adjectives, (8) conveyances, (9) kinship. Quoting Manchester, "In the three classes in which the men lead most pronouncedly, namely, in *verbs*, *implements and utensils*, and *occupations*, the notion of action is prominent. In the classes in which the women lead, such as *adjectives*, *wearing apparel and fabrics*, *interior furnishings* and *buildings and parts of buildings*, no such notion is evident. On the contrary these show a preference for things at rest." Further, the categories in which men lead show closer relation to time; those in which women lead, to space. Finally, Manchester touches on the most important trend running through both preference lists, namely, that each sex preponderates in the classifications that contain the objects and activities with which it is most concerned. The difference in the verbs used by the two groups brings this out conspicuously; men's verbs are concerned with their occupations and pursuits (examples are *shoot, tick, twitch, cure, hit, miss, aim, blow sail*), and women's with theirs (*address, telegraph, fall, rise, call, forget, ride, play, ringing, reading*). Without considering each of the classes in detail we may sum up the matter showed most interest in topics relating to personal appearance; men in topics relating to ascendancy (pugnacity, self-assertiveness, achievement). Both groups were equally interested in sex and in ethics.

Wyman includes in a report of her unique and significant *Tests of Intellectual, Social, and Activity Interests*[53] a summary of her results for sex differences. Her association test was made up of two groups of 60 words each given to 689 gifted children (IQs 140 and above) and 609 unselected children in grades four to eight, also to 156 unselected sixth and seventh grade pupils and 150 unselected seventh grade pupils. The words were selected as those most adapted to provoke responses due to intellectual interests and to social interests and activities. The lists may be described as composed entirely of (a) words in common use, (b) not too readily definable, (c) not too much tied with other words through continued association with them, (d) not words which would yield a large percentage of opposites in the responses, and (e) not words which would tend to produce carry-over effects. They were selected from a large list on the basis of actual results in a preliminary try-out. The age groups from ages 8 to 15 yrs. numbered for the gifted boys from 17 to 75 individuals, for the gifted girls from 15 to 64 individuals; the control boys (ages 10 to 15 yrs.), numbered 28 to 67 individuals per group; for the control girls (ages 10 to 15 yrs.), the age groups numbered 18 to 84 individuals. The ratings were based on qualitative valuations of the words, each word having three values established experimentally, one for intellectual, one for social, and one for activity interest.

The differences between the sexes are indicated by a comparison of averages and of variability. When in the case of all three interest categories the standard deviations are compared for the gifted boys and the gifted girls, we find that in 12 cases the measure is larger for the boys, in 12 cases for the girls. In the case of the control group the boys have a larger standard deviation in 10 of the cases, the girls in 7. With respect to the means, the gifted boys exceed the gifted girls in 13 cases; the gifted girls exceed the gifted boys in 11 cases. The means of the control boys exceed those of the control girls in 12 cases; the means of the control girls exceed those of the control boys in 6 cases. In no one of the categories and for neither of the measures (standard deviations and means) is the superiority or excess consistently in one direction. Apparently the differences in intellectual interests are not significant; those in social interest and in activity interest may be slightly so.

In contrast to all of the girls, both the gifted and the control boys show in these results a decrease in intellectual interests from 12 to 15 years. In control boys the curve of activity interest shows a moderate continuous rise, while with the control girls a fairly even level is maintained. In general Wyman's results indicate that "there are no sex differences in intellectual interest, but girls surpass boys at nearly all ages in social interest and boys surpass girls at most ages in activity interest."

The data assembled by investigators for the character of interest displayed by the sexes in word association experiments seem to show without exception that habit and occupation are important determining factors. College men and college women, school-girls and soldiers, persons of various ages, show characteristic trends. Men and boys tend to show more active and objective interests, women and girls more social and personal ones. We agree with Tanner[53] that "the experiments simply resolve themselves into a further illustration of the very well-known fact that habit determines the association of ideas," a fact that is equally attested by the different responses of persons of the same sex but of different professions.

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[53] J. B. Wyman, in Terman's *Genetic Studies of Genius*, I, 1925, chap. 16, 455-483.

(3) *Value of interest.* We have found that men have usually a somewhat wider field of mental occupation than women. Both sexes have been shown to follow habitual interests in their unreflective thought. The further question arises: does the vocabulary of one sex indicate a higher *value* of interest than the other? Are the word reactions of one sex more exact, more precise, more scientific, of better quality than those of the other? Does one sex give reactions of a higher grade than the other, word associations characteristic of better rather than of less well trained individuals? In order to test this matter as fairly and equally as possible the Rosanoffs and Martin[54] arranged a special series of 100 stimulus words drawn from the vocabulary of the science curricula of schools. They secured reactions from groups of individuals at each educational age from the fifth grade through college, adding also a group holding the Master's degree, a group holding the Doctor's degree (but not in the scientific directory), a group mentioned in American Men of Science but not starred, and one of the 'starred' individuals. All of the groups contained persons of both sexes. The reactions at each level were rated in four classes: (a) common reactions, a measure of the reactions having a value based on their appearance from average normal individuals; (b) high-standard value, a measure of the reactions having a value based on the responses of the 'starred' persons and the others listed in American Men of Science; (c) individuality index, a measure of the reactions not found in either of the foregoing; and (d) failure index, a measure of the failures to react. As to sex differences the authors found, on reviewing a table of comparison age by age from the fifth grade to the Doctor's degree, that when the sexes were rated separately on the total high-standard value, which they regard as "a more significant and more trustworthy measure of intellectual condition than any of the others made available by the test," the men made a very slightly better average showing in high-standard value in 9 and the women in 10 of the 19 groups; in other words no difference in this respect was apparent between the sexes. Furthermore the superiority of neither sex appeared more definitely at one part of the age scale than at another.

A difference was, however, noted in another regard: the men when they did not give a high-standard reaction were more apt to give a common reaction or an individual reaction; the women under these conditions were more apt to give no reaction at all. This situation was present at all ages and with all degrees of education and selection. The authors say: "Perhaps this is a manifestation of the relative passivity, conservatism, lack of venturesomeness, and other such traits popularly attributed to the female sex."

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[53] Amy Tanner, The community of ideas of men and women, *Psychol. Rev.*, 3, 1896, 548-550.

[54] Rosanoff, Martin, and Rosanoff, *op. cit.*, 1-113; especially 88 f. and 95 f.

The tendency to avoid failure of reaction by giving common or individual responses appears not only in men as compared with women, and in adults as compared with children, but also curiously enough in neuropaths as compared with normal individuals. The writers state, and we incline to agree with them, that the inhibiting mechanism acting in the case of the normal adults as compared with the abnormal is "distinct from the somewhat similar inhibiting mechanism existing in the female and which is lacking in the male sex."

Among Huber's soldier groups the old infantrymen showed more failures in reaction than did any of the other groups. These are the poorest material from the point of view of mental capacity. This tendency may be compared to that found by Rosanoff with children, and specifically with the consistent trend correlating with amount of dullness and school retardation among children of various ages. On the matter of response failures Freyd's[55] comparison of the responses of salesman and 'industries seniors' showed the latter omitting more responses than the former. The industries seniors were also more apt to be subjective, personal, and meaningful, *i.e.* more like the uncultured and feminine response types.

With reference to value of interest, the significant findings so far indicate no difference between the superficially available knowledge of groups of equal training when plumbed in the particular departments of equality. The sex difference appears in a secondary part of the reaction, namely, in the gaps between adequate knowledge; these are filled in by men with less satisfactory responses; by women with no response at all. The masculine fill-in is characteristic of the superficial, intelligent, objective type; the feminine of the meaningful, inexperienced, subjective type.

(*4*) *Normality of interest.* Normality of interest has been related to community of response by the discrete free word association method. Jastrow found that men tended to show slightly more community of interest than women on a semi-discrete test. Kent and Rosanoff[56] in a much more extended study make possible a wider comparison of sex groups. By the use of stimulus words of the non-personal sort selected by Sommer[57] from everyday experience, responses were obtained for both normal and abnormal individuals. When normal individuals, rated each as to the uniqueness of his responses, are compared in sex groups no appreciable or significant differences are found. Groups at three levels are contrasted; (a) school children, (b) persons of common school education, and (c) persons of collegiate education. The educated gave just more unique reactions than the other groups, the latter showing practically no deviation from one another. The very slight and probably insignificant group average differences show women at college level with more, and women at the common school level with fewer unique responses. These differences the authors say might disappear in another thousand cases.

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[55] M. Freyd, The personalities of the socially and mechanically inclined, *Psychol. Monog.*, 33, 1924, no. 151, 1-109.

[56] Kent and Rosanoff, *op. cit.*, 36-96; 317-390.

[57] R. Sommer, *Lehrbuch der psychopathologischen Untersuchungsmethoden*, 1899, 326-388.

Abnormal individuals of both sexes show up to four times as many individual reactions as do the normal. McClatchy[58] correlated community of response with various tests of intelligence and found that, in general, conformity to the norm or 'community of ideas' is indicative of normality, and that "the most normal person(in this sense) is the most generally intelligent." The normal individuals are those whose responses are in fact most superficial, but this superficiality "arises not from flightiness of ideas or unsteadiness of mind, but rather from a profundity of ideas and higher type of intellectuality."

The Casons[59] compared the distribution of response frequencies for 14 men and 36 women tested by a male *E* using the Kent-Rosanoff series, as against those for 20 men and 30 women tested on the same words by a female *E*. The average results for distribution of response showed no appreciable sex differences. Individuality or uniqueness of response was considerably greater for the females with the male *E*, and for the males with the female *E*. The chances out of 100 that the sex of the *E* will affect the result in the direction observed were 88, a fairly reliable indication. It was also to be expected that longer reaction-times would appear with the greater uniqueness of response. This again agrees with McClatchy's findings. Freyd's results show that community of response appears in his extraverted salesmen oftener than with his more introverted students. This would agree with the very slight difference between Kent and Rosanoff's[60] academic (presumably more introverted) and non-academic (presumably more extraverted) groups.

In their study of children, based on 300 cases, 25 at each age level, the Rosanoffs[61] found a fairly gradual increase with age from 4 to 14 in the number of common reactions, and a fairly gradual decrease with age in individual reactions and in failures of reaction. The increase in the number of common reactions seemed to correlate with increasing familiarity with the stimulus words, *i.e.* an increase in usable vocabulary. The decrease in individual reactions apparently occurred progressively as the children became mentally mature enough to mobilize attention and direct it quickly to a new stimulus word. A positive correlation was found to exist between community of association and mental capacity as rated by teachers on a 3-point scale: bright, fair, dull. Also between association and school grade a positive correlation was found which was doubtless in part due to mental capacity, but perhaps also in some extent to amount of education.

Concluding with reference to normality of interest, we find that this measure of mental maturity, adulthood, and general intelligence shows no appreciable sex difference. Small fluctuations in particular occupational or other groups are influenced perhaps by introvert or extravert characteristics.

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[58] V. R. McClatchy, A psychological study of linguistic abilities with reference to the results of word association tests, *J. Exper. Psychol.*, 7, 1924, 371-382.

[59] H. Cason and E. B. Cason, Association tendencies and learning ability, *J. Exper. Psychol.*, 8, 1925, 167-189.

[60] Kent and Rosanoff, *op. cit.*, 45, 400.

[61] Rosanoff and Rosanoff, *op. cit.*, 43-89.

(*D*) *Form Type:* (*1*) *Verbal.* In the normal discrete word association experiment Jung found little difference between the sexes in the form of the response word. Women used just more words of the same grammatical form as the stimulus, and perhaps more words of the same number of syllables. Men used more alliteration, more consonance, and more words with the same ending. With a few exceptions the same scheme of slight differences persists for all cultural groups and all types of experiment used by Jung. Under distraction the women tended to have fewer responses with the same number of syllables, the men somewhat more. Hans Huber found that the men used rather more nouns and adjectives in response, the women more verbs, adverbs and pronouns. Jastrow's and Manchester's male *S*s gave definitely more verbs than did their female *S*s. The Wisconsin (Jastrow) men exceeded the women in number of adjectives, but for Manchester's *S*s the reverse was true. In "other parts of speech" the Wisconsin men and two of three groups of California men exceeded the women. Edwin Huber's soldiers were more apt to prefer (a) adjectives, (b) verbs; Reinhold's school-girls showed the reverse tendency.

The data are corroborative of Jung's theory of the more and less meaningful types. The better educated, usually the men, tended to respond with the easier word forms, nouns, adjectives; they tended also to a certain superficiality as far as form was concerned, giving words of the same or similar sound rather than following the grammatical form. The Jung, H. Huber, E. Huber and Reinhold results seem to agree with this interpretation. The Jastrow and Manchester data do not necessarily agree, but they offer no obvious contradiction.

(*2*) *Logical.* Jastrow,[62] in the semi-discrete word association, found masculine preferences for associations by sound, from whole to part, from object to activity, from activity to object, and those by natural kind. Feminine preferences were for associations from part to whole, from object to quality, quality to object, and miscellaneous. In 1891 the same investigator[63] had found at least a strong suggestion that men favor associations by sound and from part to whole, women from whole to part and from object to quality. None of the differences is very large and all of them are dependent on subjective classification: at least in one case the results in the two investigations are apparently contradictory. Cattell and Bryant found (1889)[64] that four educated adults who teach and write favor logical and verbal associations; students favor the category whole to part; English school-girls give less than half logical and very few verbal associations; Irish school-girls give more than half logical and 8% verbal responses, perhaps as a result of their more literary training; London school-girls give about 1% verbal associations; their two teachers give respectively 33 and 26%. The two educated male adults engaged in abstract studies give three-quarters of their responses in the logical class.

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[62] Jastrow, Community and association of ideas; a statistical study, *Psychol. Rev.*, 1, 1894, 152-158.

[63] Jastrow, A statistical study of memory and association, *Educ. Rev.*, 2, 1891, 442 ff.

[64] Cattell and Bryant, Mental association investigated by experiment, *Mind*, 14, 1889, 230-250.

As Cattell and Bryant point out, their, and any similar, classification simply makes subdivision convenient; it does not necessarily indicate "natural kinds." The groups run into each other and reclassification of the same responses would not agree exactly with the figures obtained the first time. They lay no stress on the sex differences in their data. Jastrow, on the other hand, sees in the differences indicated in his study such as "may be brought into relations with recognized differences in the mental processes of men and women."

II. Classification in Terms of the Stimulus

In terms of the stimulus, *i.e.* with varying types of presentation, a few studies have brought out what have been described as sex differences in response.

TABLE XIII

Sex Differences in Terms of Percent on the Basis of Mode of Stimulus
Computed from the original data presented by H. Huber for normal *S*s

Response group	Men		Women	
	Visual	Auditory	Visual	Auditory
Inner	73.0	75.0	71.0	74.0
Outer	19.5	19.0	24.0	22.0
Clang	1.7	1.8	1.1	1.2
Residual	3.8	3.7	3.6	2.4
Egocentric	1.7	1.3	1.6	0.8
Time in sec.	2.4	2.2	2.8	2.7
Complexes	5.7	7.2	8.3	9.1
Disturbances in reprod.	21.2	22.0	17.6	20.9

(*A*) *Modal forms of presentation.* Dooley[65] and Hans Huber[66] have made comparisons of auditory versus visual presentation. Dooley's investigation made use of two different methods of (time) measurement for the two modes of presentation, so that her results are not strictly comparable for our purpose. Huber's investigation presents data for the two sexes which we have averaged as far as the normal *S*s are concerned, and present in Table XIII. The figures show that for both sexes there are more inner and clang responses with an auditory stimulus; more outer, residual and egocentric responses with a visual stimulus. In terms of our earlier discussion it would seem that the more meaningful and coöperative responses are obtained with the auditory stimulus. The tendency is barely more pronounced with men than with women, which is apparently contrary to Jung's suggestion. The results with respect to time are also contrary to Jung's findings, for the more meaningful (auditory) responses are the faster.

The educated of both sexes tend in Huber's experiment to react more rapidly to the visual than to the oral stimulus, but the reverse appears to be the case

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[65] L. Dooley, A study in correlation of normal complexes by means of the association method, this Journal, 27, 1916, 119-151.

[66] H. Huber, Ueber den Einfluss von optischem oder akustischem Reiz und grammatikalischer Form des Reizwortes auf den Assoziationsvorgang, *J. f. Psychol. u. Neurol.*, 23, 1918, 171-207.

Herrington and Willits found that women showed longer reaction-times to more words than did men. For both sexes the words responded to as complex indicators fell into definite fields of interest, and these differed for the two groups. The list was in fact, although without the investigators' intention, probably weighted to indicate more complexes for one sex than the other. The results of Hull and Lugoff, using a much longer list, showed an equal number of words that drew responses of high tension for the sexes, but again with typical content differences.

There seems to be here a question of (a) the choice of words that will elicit complexes within the masculine or feminine field of interest or mental occupation, and (b) the possible influence of the sex of the *E* which may heighten the tension of a whole series for the opposite sex. The Casons' results emphasize the latter point.

To available lists and under existing conditions, women, in the groups observed, appear to be relatively more sensitive than men; this effect is more noticeable the more diagnostic the words are of complexes. This seems to be due (a) to women's less objective response type, which is again perhaps connected with her more personalized experience, and (b) to the conditions of the experiment, where in general the *E* is a man. This related factor is further discussed in the next paragraph.

(*F*) *Influence of the E.* Sex differences due to the influence of the *E* that have been investigated and discussed pertain to reaction-time and to complexes. Jung found that women react more slowly than men, the uneducated than the educated, and although he made no control experiment with a woman investigator he traced the tendency for slower reactions primarily to emotional inhibitions and only secondarily to differences in culture: the *E* he said was in each case an educated man; for the educated men he was an individual of their own level and sex. For the uneducated men and for both groups of women he was either a person in authority placed over them, or else a person of the opposite sex, or both. This is a "sufficient reason for the universally stronger occurrence of emotional inhibitions in the remaining groups." Féré, H. Huber, Wreschner, H. Cason, Kramer and Stern[70] and other men investigators have, like Jung, found men's reaction-time in word associations shorter than women's.

Wells[71] concluded from his data that women's reaction-times grouped them into two classes, those whose responses were as short as or shorter than the men's, and those whose responses were subject to much greater deviation and averaged a longer time. This is probably accounted for by inadequate sampling. Hirszowicz[72] and W. Cason, women *E*s, found men's reaction-times longer than women's.

Herrington and Willitts found that men reacted more slowly with men *E*s than women with women *E*s in the same set-up. The Casons' data give the opposite result: both men and women responded more quickly (the men quickest of all) with a man *E*. The indications are therefore not quite clear in this matter, but tend in general to substantiate Jung's view that in word association responses men will usually react more quickly to men and women to women.

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[70] F. Kramer and W. Stern, Selbstverrät durch Assoziation, *Beiträge z. Psychol. d. Aussage*, 4, 1906, 1-32.

[71] F. L. Wells, Some properties of the free association time, *Psychol. Rev.*, 18, 1911, 1-23.

[72] S. Hirszowicz, *Experimentelle Beiträge zur Analyse des Reproduktionsvorganges*, 1909, 13-24.

With respect to complexes conditions are similar, as in fact we should conclude, since response time is itself a complex-indicator. H. Huber, Burt and Moore, Rittershaus,[73] and Hull and Lugoff, using men *E*s, find more complexes indicated for the women than the men. Haggerty and Kempf, using *E*s of both sexes, and Herrington and Willitts, using men for the men and women for the women, still find more complexes in general among the women. We are inclined to believe that there is a slight demonstrable difference between the responses of groups from the two sexes, the more so because this would seem to accord with the trends which the mental habits of the sexes at present suggest.

In this connection the work of Kramer and Stern is of interest. These investigators of word association in 'crime detection' compared the responses of 4 men (3 university students and 1 teacher) with 3 women (teachers) and found a "noteworthy" difference between the sex groups. All 4 men gave themselves away by more or less suspicious responses, "of which frequently a single one was sufficient to indicate the presence of a complex." The women gave so few 'suspicious' responses that these played only an insignificant part in the diagnosis. It should be said that the responses of one of the women were in general so much out of line with the others that the comparison is perhaps of 4 men with 2 women, and upon such small numbers conclusions as to sex differences can hardly be based. However, while drawing attention to the doubtfulness of deduction in this case, the authors quote Hans Gross as follows: "I believe you are perfectly right in thinking that women dissimulate better. A woman has altogether more to conceal than a man: menstruation, pregnancy; her interest in men she has to hide more carefully than he does his in her; in conversation she has to be more careful (modest) etc.—in short she is more accustomed to it."

Aptekmann,[74] with the collaboration of Jung, compared the responses of 6 educated men and 6 educated women with a man *E* (Jung) and a woman *E* (Aptekmann). She found (a) that the male *E* produced greater affective changes in the *S*s, (b) that where the *E* and the *S* were of the same sex more social complexes appeared; where they were of the opposite sex more erotic complexes were indicated.

Sutherland[75] sums up the matter of the *E*'s influence critically as follows: "The situation is increasingly complex as the social and administrative relations between experimenter and test person widen. And since language is essentially a social function, the conditions of the reaction are by no means confined to those usually described in reaction experiments. . . . The situation presented to a young person serving as subject for the first time for his teacher or employer contains possibilities of variation which are quite lacking to the

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[73] E. Rittershaus, Die Komplexforschung, *J. f. Psychol. u. Neurol.*, 15, 1909, 61-83, 184-220; 16, 1910, 1-43.

[74] E. Aptekmann, Experimentelle Beiträge zur Psychologie des psychogalvanischen Phänomens, *Jahrb. f. psychoanal. u. psychopathol. Forsch.*, 3, 1912, 591-620.

[75] A. H. Sutherland, *Critique of Word Association Reactions*, 1913, 1-46.

THE RANGE OF ATTENTION

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Though the problem of the 'range of attention' has a history that extends back to Aristotle, the question whether it is a proper problem to set to attention has only recently been raised. Dallenbach in 1920[1] and Fernberger in 1921[2] came to the conclusion that it is not. Both expressed the opinion, based upon experimental evidence, that the traditional problem of 'range' is one that is concerned with apprehension, not with attention.

From Aristotle to Hamilton the question of 'range of attention' has been discussed and decided upon logical and empirical grounds; and the answers returned have varied with their day and generation. Aristotle asserted that the mind is limited to the consideration of a single object,[3] *i.e.* that attention is limited to one. This dogma held for nearly 400 years, until it was rejected by Quintilian[4] who argued, from such examples as the harpist who sings as he plays his instrument, that it is possible to attend at the same instance to several sensations and objects. The schism thus made has held down to the present century.[5] The Scholastics were divided upon the question, as were also the philosophers following them. Hobbes, Locke, Wolff, Stewart, and Brown, for example, held to the Aristotelian doctrine, whereas Leibnitz, Tucker, Bonnet, and Destutt de Tracey opposed it.

Hamilton made an attempt to solve the problem by experiment. He threw "a handful of marbles on the floor" and because he found "it difficult to view at once more than six,"[6] he came to the conclusion that the constant of attention was six. Since this experiment of nearly 100 years ago, the technique has been improved, tachistoscopes have been employed to control the time of exposure, many different kinds of stimulus-objects have been used, and the experiments have been extended into the fields of audition and of touch.[7]

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[1] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 229.

[2] S. W. Fernberger, A preliminary study of the range of visual apprehension, this Journal, 32, 1921, 133.

[3] Aristotle, *Parva Naturalia, De Sensu*, chap. vii, Eng. trans. W. A. Hammond, 1902, 186 ff. Cf. also L. L. Uhl, *Attention*, 1890, 5.

[4] Quintilian, *Institutes of the Orator*, Book I, chap. xi, Eng. trans. J. Patsall, 1774, Vol. I, 66. Cf. L. L. Uhl, *op. cit.*, 7.

[5] H. Ebbinghaus, *Grundzüge der Psychologie*, 1905, 618 ff.

[6] Wm. Hamilton, *Lectures on Metaphysics and Logic*, Lect. xvi, 5th ed., I, 1870, 254.

[7] For a bibliography of the more important studies see N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this Journal, 37, 1926, 247 f.

of the *Os*' reports of one-level attensity were badly skewed with reference to them—limens in every case exceeded the largest stimulus-value used. The variation in the size of the limens among the different *Os* was, moreover, unusually large. It was perfectly obvious, therefore, that Gill and Dallenbach's work must be repeated with introspections and with an extended range of stimuli before any but provisional conclusions could be drawn.

OBJECT

The object of the present experiment is twofold. We wished in the first place to repeat Gill and Dallenbach's work, adding introspections and extending the range of the stimulus-values; and in the second place to demonstrate that the constant of the traditional 'range of attention' experiment is dependent upon the degree of cognition involved in the report, and that the range of *attention*—if it be possible to formulate such a problem—can be properly investigated only when the cognitive factor is eliminated.

PROCEDURE

Five series of tachistoscopic experiments were performed; the *Os* being differently instructed in every series. In the first series, Experiment I, in which we sought to escape the cognitive complications of the traditional experiments, we instructed our *Os* to report merely upon the distribution of the attensity of their impressions during the moment of exposure. In the remaining series, Experiments II–V, the cognitive complications involved in report were taken into account. We induced different levels or degrees of cognition by instructions and observed how the 'range' of the reports was affected. We instructed the *Os* in one series to give the number of stimulus-objects exposed, in the next series to name the letters exposed, in the next series to name the forms exposed, and in the last series to describe the forms exposed. We began our study with a series of experiments that did not involve cognition (at least not the cognition of the stimulus-objects), and progressed to series that demanded higher and higher levels of cognition for the reports, because we thought that the 'habits' of report would be weaker and less inhibitory if we passed from lower to higher degrees of cognition than if the progression were reversed.

Observers. The *Os* were Miss Alexandra Hobart (*H*), a senior majoring in psychology; Mr. Tallmadge Griffin (*G*) a graduate student in psychology, and the senior author (*D*). They served three times a week and at the same hour on the different days. *D* was an *O* in Gill and Dallenbach's study and he had also served in a number of other studies on attention. He was the only one of our *Os* who came to the study introspectively familiar with the dimension of attensity.

Preliminary experiments. Because *G* and *H* were introspectively unfamiliar with attensity (attributive clearness, vividness) we gave them a series of preliminary experiments which had been devised in the Cornell laboratory for training in the introspection of it.[18] The experiments were continued until the *Os* were able to note the grosser variations of attensity; until they were able, for example, to characterize certain impressions as more insistent, protruding, clearer, or more vivid than others. When this point had been reached we turned to the main experiments. *H* quickly reached this degree of proficiency, but *G* was more perverse.[19] It was only after long practice that he gave any indication that he appreciated the attensive differences in experience.

While *G* and *H* were being given the preliminary training, *D* was observing in the main experiments. The first series of the main experiments (Experiment I)—those involving the report of attensity—were given to *D* twice over before *G* and *H* were ready for them. The first time they were given with a dark post-exposure field; the second time with a light post-exposure field identical with the pre-exposure field.

These experiments were conducted in order to discover whether the quality of the post-exposure field would affect the results. We wished to know, so as to be guided in conducting the experiments with the other *Os*, whether there were fewer or more positive and memory after-images with a light than with a dark post-exposure field, and also whether the presence of these after-experiences would render reports of attensity more or less difficult.

The results of these two series of experiments with *D* are given in Tables IA and IB below. There is very little difference between the results of the two series; but what difference does exist tells in favor of the light post-exposure field. The objective results show that the limen is slightly larger and the precision slightly greater when the light field was used. *D*'s introspections also show that the light field was probably the better. He reported that positive and memory after-images were more intense and of more frequent occurrence when the dark post-exposure field was used than when the light field was used. Though he was of the opinion that these after-images affected neither his experience nor his report, their increase in intensity and frequency was sufficient to prejudice us against the dark field. We therefore used the light post-exposure field for *G* and *H* in Experiment I, and for all the *O*s in the remaining experiments (Experiments II–V).

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[18] Cf. K. M. Dallenbach, The measurement of attention, this JOURNAL, 24, 1913, 467.

[19] *H* came to the preliminary experiments merely naïve in the introspection of attensity, but *G* came to them with a certain amount of sophistication and a prejudice against the concept of attensity as an attribute or dimension of experience. It may have been unwise to have included among our *Os* one of such strong persuasions who was thus negatively conditioned, but we gladly accepted him as a challenge. The selection of *Os* is doubtlessly justified, but it is our opinion that it often leads to the stereotyping of results, and to the indefinite postponement of important discoveries. Anomalous reports of 'nonconforming' *Os* have frequently in the history of psychology been of more consequence than the routine reports of the 'regular' *Os*.

Apparatus. For the exposure apparatus we used throughout all five series of experiments the Dodge tachistoscope,[20] together with a modification of Crosland's timing device.[21] The time of exposure was approximately 78 sigma.[22] As described by Crosland his apparatus gives a dark post-exposure field. As this was found to be undesirable (cf. *supra*) we modified the apparatus to give a light post-exposure field identical with the pre-exposure field.

Materials. The exposure-cards were 11.5 x 5.5 cm.—the size adapted to the Dodge instrument. They were white, with a soft, dull surface. The stimulus-objects were black (with the exception of those used in Experiment 5, which were colored) and (with the exception of those used in Experiment 3, which were letters) they consisted of small geometrical forms which were cut out of the cards by punches. When the cards were placed in the holder, which was painted a dead black, the stimulus-objects were uniform in appearance and sharp and definite in outline.

Care was taken in making the cards to eliminate as far as possible the effect of position and of pattern. The stimulus-objects were distributed evenly over the exposure-field, and the cards which showed an objective grouping were rejected.

EXPERIMENT I

We hoped in Experiment I to escape the cognitive complications of the traditional experiments by instructing our *O*s to report, not upon the number or kind of stimulus-objects observed, but upon the distribution of the attensity of their experience, *i.e.* whether the impressions were at one, two, or more levels of attributive clearness. What we sought in this experiment was the answer to the question how many impressions may, in a single consciousness, be experienced at the upper level of attributive clearness; or to put the question more concretely: what is the limen of the upper level, how many stimulus-objects may be placed upon an exposure-card before they are experienced 50% of the time at two or more levels of clearness.

Exposure Cards. Ten series of exposure-cards were prepared in the manner described above. Every series was composed of six cards which had respectively 3, 8, 13, 18, 23, and 28 stimulus-objects. In the first nine series (Series A–I) a single form was used in a given series; in the tenth series (Series J) the forms were all different in the cards with 3, 8, and 13 stimulus-objects, and no form was used more than twice in the cards with 18, 23, and 28 stimulus-objects. The forms used in Series A–I were the same as those used by Gill and Dallenbach[23]—they are listed in Table IA below. In Series J all the forms of the first nine series and five new ones were used.[24]

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[20] R. Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10-13.

[21] H. R. Crosland, A device for controlling the time of exposure in the Dodge tachistoscope, *J. Exper. Psychol.*, 9, 1926, 162-168.

[22] The average of 10 trials was 78.29 ± 2.27σ.

There were thus 10 cards for every stimulus-value or 60 exposure-cards in all. The cards were shuffled and presented in haphazard order, and the procedure was repeated 10 times, making a total of 100 observations for every stimulus-value, or 600 observations for very *O*.

Instructions. The following instructions, identical with those used by Gill and Dallenbach, were read to the *O*s at the beginning of every experimental hour.

> "At the signal 'Ready,' fixate the black dot on the pre-exposure field. At 'Now' you will be shown a white field upon which there is a number of black figures. After the experiment report upon the attensity, that are the clearness or vividness of the impressions, during the exposure period. You may use the following terms: 'one' or 'equal,' meaning that all the impressions were at one level of attensity; 'two' or 'different,' meaning that two levels were observed; and 'three' or 'multiple,' meaning that three or more levels were observed."

Doubtful judgments were permitted, but the experiments in which such reports were given were later, without *O*'s knowledge, repeated. The experiments were interrupted at irregular intervals and the *O*s were asked to give an introspective report upon their experience during the exposure-period. They were also instructed to interrupt the course of the experiments any time they had anything important to report.

Results. The *O*s' reports were either 'one' or 'two.' Not once in the entire experiment was the report of 'three' returned, though it was, as in the earlier study by Gill and Dallenbach, tacitly assumed and provided for in the instructions. The *O*s' introspections show that their experiences, during the moment of exposure, were divided into two levels of attensity: a clear focus and an obscure background.[25] In the experiments in which the report of 'one' was given, the impressions on the exposure-card were all experienced at the upper level. At the lower level there were certain obscure kinaesthetic and organic sensations and images. When 'two' was reported, the impressions from the exposure-cards were themselves divided into two levels of attensity: some were

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[23] *Op. cit.*, 250.

[24] Greek cross, heart, rectangle, angle, and an irregular form that the *O*s called a 'crown.'

[25] Cf. E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 220-242; K. M. Dallenbach, The measurement of attention, this JOURNAL, 24, 1913, 506 f; Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 225-227; N. F. Gill and K. M. Dallenbach, *op. cit.*, 252.

TABLE IA

SHOWING THE NUMBER OF TIMES THAT THE IMPRESSIONS FROM THE VARIOUS STIMULUS-SERIES WERE REPORTED AT ONE LEVEL OF ATTENSITY, THE TOTAL NUMBER OF THESE REPORTS FOR EVERY FORM-SERIES, THE AVERAGE, AND THE DEVIATION FROM THE AVERAGE

O	Post Ex-posure field	Form Series	Forms	Number of Stimulus-objects						Total Form-Series	Dev. from Av.
				3	8	13	18	23	28		
D	Dark	A	Small disks	10	10	5	3	2	3	33	− 1.6
		B	Medium disks	10	10	8	2	1	1	32	− 2.6
		C	Large disks	10	8	7	1	3	3	32	− 2.6
		D	Semi-disks	10	10	10	9	2	0	41	+ 6.4
		E	Triangles	5	10	8	2	5	0	30	− 4.6
		F	Clubs	10	6	6	2	1	2	27	− 7.6
		G	Diamonds	10	7	7	7	2	3	36	+ 1.4
		H	Squares	10	6	7	3	3	2	31	− 3.6
		I	Spades	10	9	9	7	1	3	39	+ 4.4
		J	Combinations	7	10	9	9	6	4	45	+10.4
			Total	92	86	76	45	26	21	Av. 34.6	± 4.5
D	Light	A	Small disks	10	9	7	7	0	1	34	− 3.1
		B	Medium disks	10	10	8	7	1	2	38	+ 0.9
		C	Large disks	10	8	8	4	1	1	32	− 5.1
		D	Semi-disks	9	10	10	6	7	3	45	+ 7.9
		E	Triangles	9	10	5	3	3	2	32	− 5.1
		F	Clubs	10	10	8	0	2	0	30	− 7.1
		G	Diamonds	10	9	9	3	1	2	34	− 3.1
		H	Squares	10	8	9	5	3	1	36	− 1.1
		I	Spades	10	10	8	6	1	2	37	− 0.1
		J	Combinations	10	10	10	7	8	8	53	+15.9
			Total	98	94	82	48	27	22	Av. 37.1	± 4.9
G	Light	A	Small disks	10	10	9	10	10	10	59	+ 0.5
		B	Medium disks	10	9	9	10	10	10	58	− 0.5
		C	Large disks	10	10	10	9	10	10	59	+ 0.5
		D	Semi-disks	10	10	10	9	10	10	59	+ 0.5
		E	Triangles	8	10	10	10	10	10	58	− 0.5
		F	Clubs	9	9	9	10	10	9	56	− 2.5
		G	Diamond	10	10	10	10	10	10	60	+ 1.5
		H	Squares	10	10	10	10	10	9	59	+ 0.5
		I	Spades	10	9	9	10	10	10	58	− 0.5
		J	Combinations	10	10	10	10	9	10	59	+ 0.5
			Total	97	97	96	99	98	98	Av. 58.5	± 0.8
H	Light	A	Small disks	8	5	6	9	5	5	38	+ 7.1
		B	Medium disks	8	5	5	3	2	4	27	− 3.9
		C	Large disks	8	7	8	9	9	8	49	+18.1
		D	Semi-disks	5	2	2	6	6	3	24	− 6.9
		E	Triangles	6	0	9	4	10	6	35	+ 4.1
		F	Clubs	0	1	3	6	6	6	22	− 8.9
		G	Diamonds	6	9	7	5	7	5	39	+ 8.1
		H	Squares	1	3	5	6	1	2	18	−12.9
		I	Spades	2	6	2	3	6	6	25	− 5.9
		J	Combinations	6	0	4	7	8	7	32	+ 1.1
			Total	50	38	51	58	60	52	Av. 30.9	± 7.7

clear, others were obscure. Since these two kinds of reports are reciprocal, we give in Tables IA and IB the number and distribution of the reports of 'one' only.

Table IA shows the number of times that the impressions from the various stimulus-series were reported at one level of attensity; Table IB gives the percentages of the one-level reports for every stimulus-value, and the limens and measures of precision for the two series of experiments with *D*—the only one of our *O*s for whom a limen could be calculated. The results are very different for the three *O*s; we must, therefore consider, them separately.

TABLE IB

SHOWING FOR EVERY *O* THE PERCENTAGE OF ONE-LEVEL REPORTS FOR EVERY VALUE OF STIMULUS, AND THE LIMEN (L), AND THE MEASURE OF PRECISION (h) FOR THE TWO SERIES OF EXPERIMENTS WITH *D*

	Number of Stimulus-objects							
O	3	8	13	18	23	28	L	h
*D**	92	86	76	45	26	21	18.2	0.07
D†	98	94	82	48	27	22	19.4	0.08
G	97	97	96	99	98	98	——	——
H	50	38	51	58	60	52	——	——

*Dark post-exposure field †Light post-exposure field

The frequency of *D*'s one-level reports varies, in both series of experiments conducted with him, with the number of stimulus-objects exposed. The curves of his reports are smooth and regular, with no inversions of the first order.[26] The limens, calculated from these data by the method of constant stimuli, are 18.2 for the experiments with the dark post-exposure field, and 19.4 for those with the light post-exposure field. These values agree closely with the liminal value, 17.3, obtained for *D* in Gill and Dallenbach's earlier study.[27] Do these 'constants' represent *D*'s 'range of attention'? It is obvious that his reports are conditioned by the number of stimulus-objects on the exposure-cards, but we must, before we are justified in drawing this conclusion, be assured that 'number' was the only condition that affected his reports. The introspections do not help us to determine this point, for different 'set.' Unless, therefore, 'set' be specifically defined in every case explanation is bankrupt. Our results tell us nothing about 'set' beyond the fact that it differed among our *Os*. We are, consequently, unable to explain our results. An explanation is not, however, necessary or even pertinent to the purpose of the present experiment. The significant thing for us is that the *Os*, reports of 'one' were differently conditioned. That fact—with its corollary, that 'number' of stimulus-objects exposed was not a compulsory condition, indeed, that for two of our *Os* it was not even a contributory condition—indicates that 'range' is not a question that can properly be asked of the attentive integration in these experiments. The *Os*' attitude or 'set,' not the 'number' of stimulus-objects exposed, determined the report returned.

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[26] These results sustain the opinion expressed by Gill and Dallenbach (*op. cit.*, 252) that a smooth psychometric curve would be obtained in their experiments if the number of stimuli were greatly extended above 15, and if stimulus-values were selected which differed by steps of 5 instead of by 1.

[27] *Op. cit.*, 252. The agreement is surprisingly close when it is recalled that the value in the earlier study was calculated from an upper section only of the psychometric curve.

EXPERIMENT II

We sought in the second series of experiments to determine the number of stimulus-objects that could be apprehended correctly. We thought that the report of mere 'number' would involve as low a degree of cognition as we should be able to attain in the tachistoscopic experiment.

Exposure-cards. Ten series of 13 exposure-cards, whose stimulus-values ranged from 3–15 stimulus-objects, were prepared in the manner described above. The stimulus-objects, with one exception, were the same as those used in Experiment I.[30] A different form was used in every one of the first nine series. All those forms and six additional ones were used in the tenth series.[31] Care was taken in making the cards of the tenth series to see that no form was used more than once on any given card, and that every form was used on the different cards approximately as often as every other form.

The cards were shuffled and presented to the *Os* in haphazard order, and the procedure was repeated 10 times. Thus 100 reports were obtained for every value of stimulus—a total of 1300 reports for every *O*.

Instructions. The following instructions were read to the *Os* at the beginning of every experimental hour.

"Cards will be exposed which contain a varying number of black forms on a white background. Immediately after the exposure report the number of forms you have apprehended. Be sure of your judgment; do not guess. The report, 'I do not know,' is admissible."

At the end of every experimental hour the *Os* were asked to give an introspective account of their experience during the exposure and of the conditions underlying their subsequent reports. They were also told to interrupt the course of the experiments—which they did—whenever they wished to give an introspection.

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[30] A small disk with a projecting bar was substituted for the large disk. The *Os* designated this figure as 'Saturn.'

[31] The five additional forms used in Experiment I and a new form, a disk with a white band, which the *Os* called 'Globe.'

Results. We give the objective results in Table II, which shows, for every *O*, the frequency with which the number of stimulus-objects exposed was correctly apprehended, together with the limens and measures of precision as calculated by the method of constant stimuli.

TABLE II

SHOWING THE FREQUENCY WITH WHICH THE *O*s CORRECTLY REPORTED THE NUMBER OF STIMULUS-OBJECTS EXPOSED, THE LIMENS (L), AND MEASURES OF PRECISION (h)

O	Number of Stimulus-objects													L	h
	3	4	5	6	7	8	9	10	11	12	13	14	15		
D	100	100	100	100	97	84	83	74	68	58	5	8	3	11.3	0.30
G	100	98	78	49	38	3	2	4	0	0	0	0	0	6.2	0.44
H	100	100	100	98	98	63	55	16	3	2	0	0	0	9.2	0.53

Though the *O*s' limens differ greatly in size, being 6.2, 9.2, and 11.3 for *G*,[32] *H*, and *D* respectively, the frequency curves of their reports are similar except for the aspect of spread. The curves of all the *O*s show a slow and gradual decline as the number of stimulus-objects increases until a point is reached—at 13 for *D*, 8 for *G*, and 10 for *H*—at which there is a sharp break. Both the size of the limen and the sharp breaks in the frequency curves are due, we believe, to the methods that the *O*s used in arriving at a report.

When the stimulus-objects were few in number, the impressions were apprehended as separate units and the reports were immediately returned: that is, as the following introspections show, the reports were touched off by the 'given' without the mediation of other conscious processes.

D, [3 stimulus-objects exposed; 3 reported]: "Judgment immediate, based upon impression which was at upper level of clearness." [4 stimulus-objects exposed; 4 reported]: "Impressions equally clear, and immediately apprehended. Memory after-image did not appear until after the report."

H, [4 stimulus-objects exposed; 4 reported]: "Lower two forms clearer than upper. Did not have to count them, that is, answer immediately given by exposure."

When more stimulus-objects were exposed the impressions were apprehended as a group, and the reports were returned upon the basis of this group apprehension without the mediation of other experiences.

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[32] For a further discussion of *G*'s limen in this experiment see p. 223 f.

point, and as every stimulus-object had the same opportunity of being seen, we had no choice but to count all reports that did not agree with the number of objects exposed as incorrect.

The introspections also show that clearness of experience cannot be correlated with accuracy of report. There are many instances, illustrated by the following excerpts, in which the stimulus-objects were experienced at a high degree of attensity and the reports were nevertheless incorrect:

D, [15 stimulus-objects exposed; 13 reported]: "Impressions equally clear during the exposure."

H, [general statement]: "Sometimes the forms [*i.e.* the stimulus-objects] stand out clearly and distinctly in experience and yet I am unable to report the number exposed."

Converse instances, *i.e.* correct reports when the stimulus-objects were divided in experience between two levels of attensity, occurred occasionally as the following introspections illustrate:

D, [10 stimulus-objects exposed; 10 reported]: "During the exposure the impressions were divided into two levels, the center of the field at the upper level and the margin at the lower. The report of 10 based upon the sum of three groups formed in the memory after-image, upon a group of 5 in the middle of the field, of 3 in the left, and of 2 in the right."

H, [4 stimulus-objects exposed; 4 reported]: "Lower two forms [*i.e.* stimulus-objects] clearer than the two at upper right."

It is clear in view of these findings that experience and report are not unvaryingly correlated: a clear experience is not a guarantee of a full report, nor is a full report a guarantee that experience has been clear.

EXPERIMENT III

In the third series of experiments we sought to determine the 'range' at the level of cognition required for naming.

Exposure-cards. Letters were used in this series as the exposure material. They were printed by hand with pen and ink in capitals 1 cm. high. Because of the confusing similarity of C and G, E and F, O and Q, P and R, V and U, and W and M, the second members of these pairs were not used; our exposure material was chosen from the remaining 20 letters of the alphabet. The number of letters printed on the different cards varied from 4 to 9. Twenty cards were prepared for every stimulus-number, giving for the 6 series a total of 120 cards. The letters were printed in two rows in the center of the exposure-cards. When an even number of letters was used, they were arranged in two equal lines; when an odd number was used the extra letter appeared in the first line and the second line was centered under the first. Care was taken in preparing the cards to use the letters equally often, to place them in one position as frequently as in another, and to arrange them so as to avoid words, meaningful syllables, and abbreviations.

The cards were shuffled and given to the *Os* in haphazard order, and the procedure was repeated 5 times; thus 100 reports were obtained for every stimulus-value, or a total of 600 reports from every *O*.

Instructions. The following instructions were read to the *Os* at the beginning of every experimental hour.

> "You will be shown cards which will contain a varying number of black letters on a white background. Immediately after the exposure name the letters that you have seen. Be sure of your judgment; do not guess. Report as many as you can even though you may not be able to name all."

Introspections were asked for at the end of every experimental hour, and they were permitted at any time during the experiments that the *Os* wished to give them.

Results. We give the objective results in Table III, which shows the number of correct reports for every stimulus-value, and the *Os*' limens and measures of precision computed from these data by the method of constant stimuli. We counted a report as correct if the letters were, irrespective of the order of their naming, named correctly. We gave no credit for partial successes; a report was either right or wrong. None of the *Os* named more letters than were exposed. Their errors were errors of omission or naming.

TABLE III

SHOWING THE NUMBER OF TIMES THAT THE *Os* CORRECTLY NAMED THE LETTERS ON THE DIFFERENT EXPOSURE-CARDS, THE LIMENS (L), AND THE MEASURES OF PRECISION (h)

	Number of Letters							
O	4	5	6	7	8	9	L	h
D	98	98	97	74	49	19	7.9	0.43
G	100	100	90	45	11	0	6.9	0.88
H	100	90	33	14	0	0	5.9	0.82

D's limen for naming is the largest among our *Os* (7.9), *G*'s is second in size (6.9), and *H*'s is the smallest (5.9). The rank order of the limens of *G* and *H* is just the reverse of that obtained in Experiment II. *D*'s and *H*'s limens show a marked decline in this experiment, being lower by 3.4 and 3.3 units respectively than in the preceding series. *G*'s limen, on the contrary, is higher in this series by 0.7 of a unit.

The discrepancy between the results of *D* and *H* on the one hand and of *G* on the other is due to *G*'s interpretation of the instructions. "Be sure of your judgment; do not guess" meant very different things for him in the two series of experiments. He says, in one of his introspections of this series of experiments, "I have taken 'guess' in the instruction in the literal sense and not in the sense that I took it in the previous experiment where I regarded it as the come rapidly and freely as many as 7, 8, and 9 letters may be named correctly by *H*, *G*, and *D* respectively before the memory after-image is lost; but if the associations are inhibited, for any one of a number of reasons, and the memory after-image disappears, the processes of cognition are interrupted. In cases of inhibition the *Os* do one of five things.

(a) They give up; their reports are interrupted at the point of the inhibition. Examples are:

D, [9 letters exposed; 1 correctly named]: "Reported H but knew it was B; intended to say B. This error and its correction inhibited cognition of the rest of the letters."

H, [9 letters exposed; 4 correctly named]: "Reversal of position of H and J delayed me to such an extent that the memory after-image disappeared before I could proceed with the naming."

(b) They change their usual order of report. Examples are:

D, [6 letters exposed; 6 correctly named]: "Reported last line first because I was held up by the first letter of the first line." [8 letters exposed; 8 correctly named]: "Intended to report upper line first, but W, the first letter of the last line, protruded and it was involuntarily pronounced, so I reported last line first and then returned to first line."

(c) They carry the meaning of the inhibited item in other terms, and translate it after the formal report. Examples are:

D, [9 letters exposed; 1 correctly named]: "Reported H but knew it was a B." [6 letters exposed; 6 correctly named]: "Called the V a Z although I knew it was a V."

H, [6 letters exposed; 6 reported]: "Said B instead of V for the first letter."

(d) They curtail their reports. Examples are:

D, [7 letters exposed; 5 correctly named]: "Inhibition in first line . . . retarded naming to such an extent that I practically lost lower line."

H, [8 letters exposed; 6 correctly named]: "Lost last two letters because I failed to name X in its correct position after the Z."

(e) Or they skip the letter causing the inhibition. Examples are:

D, [8 letters exposed; 7 correctly named]: "Named first three letters in upper line, skipped the last one because name did not come easily, went to bottom line and named across." [5 letters exposed; 5 correctly named]: "Could not think of S so went to O and then back to S at the end."

H, [6 letters exposed; 5 correctly named]: "Inhibition made me lose the middle letter of last line."

Among the various causes of inhibition the *Os* reported the following.

(1) Transpositions in the order of naming the letters. Not all the transpositions, however, caused inhibitions. Many were made, as the objective records show, that the *Os* were not aware of. Awareness of a transposition appeared to be necessary to cause the report to be inhibited.

D, [9 letters exposed; 3 correctly named]: "Transposed second and third letters; that inhibited the naming of the rest."

H, [9 letters exposed; 4 correctly named]: "Reversal of position of H and J delayed me to such an extent that the memory after-image disappeared before I could proceed with the naming."

(2) The arrest of attention by some aspect in the experiment.

D, [8 letters exposed; 0 reported]: "Second letter in bottom row caught my attention. Though it was the clearest item of experience I could not name it, neither could I name any of the other letters. It inhibited them all."

(3) The feeling of surprise.

D, [8 letters exposed; 4 correctly named]: "Perceived all 8 letters very clearly and felt certain that I could name them. Named top row from left to right. When I turned to name the first letter of bottom row found to my surprise that it was the same letter in cognition as the first letter of the first row. This feeling of surprise inhibited further cognition and I was unable to tell what the rest of the letters were although I am certain that they were as clearly perceived as the letters named."

(4) The misnaming of letters.

D, [8 letters exposed; 4 correctly named]: "Misnamed second letter, corrected it. This inhibited me sufficiently long to interfere with correct cognition of letters toward end of series."

(5) Mental block.

H, [8 letters exposed; 4 correctly named]: "For some reason I could not start my report immediately after the exposure. This delay made me forget the last line."

(6) Stumbling in the pronunciation of a letter.

D, [7 letters exposed; 4 correctly named. S and Z last two letters in upper row]: "Rapid pronounciation of S and Z caused me to stutter—an inhibition sufficiently disturbing to obliterate partially the memory after-image of the letters of the lower row."

(7) Extraneous associations.

H, [6 letters exposed; 6 correctly named]: "Top line A C H. The letters of my high school, A C H S, immediately came to mind. This inhibition nearly made me lose the last line." [5 letters exposed; 5 correctly named]: "The letters of the top line are the initials of a friend; the thought of that almost made it impossible to give the rest of the report."

EXPERIMENT IV

The exposure-materials of Experiment III were familiar and a high degree of cognition was not necessary for their report. In the present series of experiments, which is practically a repetition of the third, we sought to induce a higher degree of cognition than that required in the third series by using less familiar exposure-materials. We wished in particular to learn what effect familiarity with the exposure-material has upon the 'range' and upon the conditions affecting the report.

Exposure-cards. The geometrical forms used as the exposure-materials in Experiments I and II were again used in this series of experiments.[18] One hundred exposure-cards, 20 for every stimulus-number from 2–6,[19] were prepared in the manner described above. Care was taken in making the cards to see that the forms were scattered evenly over the exposure-field (as fre-

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[18] We used specifically the 15 forms of Experiment II.

[19] We decided upon these limits because we found, in a preliminary series of experiments with *D* in which from 3–9 of these geometrical forms were exposed, that he was unable to name all of the forms on a card correctly when the higher stimulus-values were presented.

Exposure-cards. Twelve of the geometrical forms used in Experiment IV were again used in the exposure-cards of this series.[40] Sixty cards were prepared, 20 for every one of the three stimulus-values, 2, 3, and 4, in the manner described above. The forms were concentrated near the center of the exposure-field and care was taken to avoid arranging them into objective patterns. Every form was used an equal number of times in the series. None was used more than once on the same card. After the forms had been punched into the cards, small squares of colored paper were glued over the holes so that the cards from the exposed side showed a number of differently colored forms. Five colors (red, orange, yellow, green, blue) and black were used.[41] These qualities were used equally often in the series, and no quality was used more than once on the same card. The exposure-cards were shuffled and presented in haphazard order 5 times, making a total of 300 observations, 100 for every stimulus value, for every *O*.

TABLE V

SHOWING THE NUMBER OF TIMES THAT THE *O*s CORRECTLY DESCRIBED THE DIFFERENT EXPOSURE-CARDS, THE LIMENS (L), AND THE MEASURES OF PRECISION (h)

	Number of colored forms				
O	2	3	4	L	h
D	64	52	17	3.3	0.45
G	88	59	4	3.0	0.97
H	92	67	18	3.3	0.83

Instructions. The following instructions were read to the *O*s at the beginning of every experimental hour.

"You will be shown cards which contain a varying number of colored forms on a white background. Immediately after the exposure describe the forms you have seen, that is, give the number, names, and qualities of the stimulus-objects. Be sure of your descriptions; do not guess."

At the end of every experimental hour the *O*s were asked, as in the other experiments, to give an introspective report. They were also permitted as before to give introspections in the course of the experiments at any time they wished.

Results. We give the objective results in Table V, which shows the number of times that the *O*s correctly described the exposure-cards, and the limens and measures of precision computed from those data by the method of constant stimuli. We counted a description as correct if the *O*s named all the forms and their respective qualities correctly. As in the other experiments, we did not credit partial successes: a report was either right or wrong. If, as frequently occurred, the *O*s named the forms and qualities correctly but failed to correlate them correctly, we counted the report as wrong.

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[40] Dot, disk, 'Saturn,' globe, semi-disk, angle, triangle, square, heart, spade, Greek cross, and diamond. The club, rectangle, and 'crown' were omitted from the previous series.

[41] We used specifically the following colors: the 'Farbenkreis Rot,' *i.e.* the 'Hering Red,' Stoelting color no. 1; the Milton-Bradley 'Orange;' the 'Farbenkreis Gelb,' *i.e.* the 'Hering Yellow,' Stoelting color no. 5; the Milton-Bradley 'Green;' the 'Farbenkreis Blau,' *i.e.* the 'Hering Blue,' Stoelting color no 13. Nothing was pasted over the holes we wished to appear 'black.' The surface of the card-holder sufficed for that quality.

The 'descriptive' limens are 3.3, 3.3, and 3.0 for *D*, *H*, and *G* respectively. *H* and *D* are tied, and *G* is but slightly below them. The limens in this series of experiments are smaller than those of Experiment IV by 1.0 for *D*, 0.6 for *H*, and 0.3 for *G*. These differences, except perhaps the difference for *D* (which is, as we show below, enlarged by our arbitrary treatment of the data), are surprisingly small; we had expected that the limens for naming the 'form and quality' would be much smaller than for naming the 'form' alone. The differences, however, though small are constant and, due to the large number of cases involved in the two experiments, significant. We feel justified, therefore, in concluding that degree of cognition required by the instructions is another factor in determining the *O*s' reports.

In addition to the errors of correlation mentioned above, the *O*s' errors were for the most part errors of omission and qualitative description. Very few of the geometrical forms were reported incorrectly and no errors of addition of either forms or colors were made. The *O*s showed striking individual differences, however, in the kind of errors that they made: 79% and 73% respectively of *G*'s and *H*'s errors were errors of omission, whereas only 9% of *D*'s errors were of that kind. *D*'s errors were principally errors of correlation and of color naming. The difference between *G* and *H* on the one hand and *D* on the other may be due in part to a difference in their method of reporting. *G* and *H* gave the quality with the form as "red square, green diamond, black triangle," but *D* seldom gave his reports in that fashion. He named the forms first and then the colors, or contrariwise the colors first and then the forms. He adopted this method of reporting, as the following excerpt from one of his introspections shows, because he found that the transition from one aspect of the report to the other inhibited his associations more than if he completed his report of one aspect before taking up the other. He says:

> "The name associations are not inhibited so much if I name the colors and then the forms, or vice versa, but when I transfer from one aspect to the other the report is difficult and halting. I do not consciously, however, set myself to report one or the other aspect first."

(2) That, in the 'cognitive' series of experiments, the range, defined as that value of stimulus which has a 50% probability of being correctly reported, varies (a) inversely with the degree of cognition required for the report;

We used the same exposure materials (geometrical forms) in Experiments II, IV, and V, with different instructions. We asked the *Os* in Experiment II to report the number of stimulus-objects exposed, in Experiment IV to name them, and in Experiment V to describe them. The 'ranges' in these three experiments were 11.3, 4.3, and 3.3 respectively for *D*; 9.2, 3.9, and 3.3 respectively for *H*; and 6.2, 3.2, and 3.0 respectively for *G*—thus clearly establishing the fact that the higher the degree of cognition the smaller is the range of report, and contrariwise, the lower the degree of cognition, the higher is the range of report.

(b) with the kind of material exposed;

We used the same instructions (to name) in Experiments III and IV, but different exposure materials. In Experiment III we used letters, and in experiment IV geometrical forms. The 'ranges' in these two experiments were 7.9 and 4.3 respectively, for *D*, 6.9 and 3.2 respectively for *G*, and 5.9 and 3.9 respectively for *H*.

(c) inversely with the degree of assurance or certainty of the *O*'s report;

The effect of the degree of certainty or assurance upon the report is demonstrated in the results of but one *O* (*G*), and by him only accidentally. *G* rigidly interpreted the instruction "do not guess" in one series of experiments (Experiment II) to mean 'do not report unless absolutely certain;' in another series of experiments (Experiment III) he took 'guess' in its "literal sense," and reported everything about which he was reasonably certain. In the series in which he was 'absolutely certain' of his reports his range was 6.2; in the series in which he was 'reasonably certain' his range was 6.9. The effect of the differences in certainty is greater than these results show, for these results are cut across and partially obscured by the differences in the degree of cognition involved in the reports. The experiments involving the lower degree of cognition were reported under the limitations of the highest degree of assurance, those involving the higher degree of cognition were reported at a lower degree of assurance. The fact that an increase in the degree of certainty showed its effect, under these circumstances, by a decrease in the 'range' indicates that for *G* at least the degree of assurance required for a report is more effective in determining the range of that report than the degree of cognition.

(d) with the duration of the memory after-image;

The introspections of all the *Os* show that the memory after-image plays an important rôle in all of the cognitive experiments. The longer the duration of the memory after-image the more extensive is the report, and the greater the degree of cognition required for the report the more necessary is the after-image to the report.

(e) with the facility of verbal association and the freedom from associative inhibitions;

Both the introspective and objective results show that the greater the degree of cognition required for the report, and the shorter the duration of the memory after-image, the more important are the verbal associations and the more injurious to the report are the associative inhibitions. The *O*s mentioned the following causes of inhibitions in their introspections: transpositions of the usual or intended order of report, distractions, feelings, misnamings, stuttering, mental block, and extraneous associations. The list is by no means exhaustive, the *O*s were not 'set' to report the causes of their inhibitions. The causes listed are only those that were incidentally reported. When inhibitions occurred the *O*s either gave up, changed their usual order of report, carried the meaning of the inhibited association in other terms, curtailed their reports, or skipped the inhibited item and continued the report.

and (f) with the color sensitivity of the *O*s.

One of our *O*s (*D*), is partially color-blind. In Experiment V, in which reports of color were required, 97% of his incorrect reports were due to errors involving color.

(3) That the *O*s' reports are based upon two types of apprehension: (1) an immediate type which is divided into two subtypes: (a) 'unit' apprehension, and (b) 'group' apprehension; and (2) a mediate type which is dependent upon re-imaging and other ancillary processes.

When a low degree of cognition is required (as in the cognition of number in Experiment II), or when the stimulus-material is relatively simple (as the letters in Experiment III) and few in number, the *O*s' reports are based upon what we have chosen to call 'unit' apprehension. The impressions are apprehended as separate units and the reports are immediately touched off by the 'given' without the mediation of other conscious processes. When more stimulus objects are exposed or the stimulus conditions are a little more complex, the *O*s' reports are based upon 'group' apprehension. The impressions are apprehended as a group and the reports are immediately made as before without mediating experiences. When the higher degrees of cognition are required, or more complex stimulus-materials are used, the *O*s' reports are mediated by various experiences.

(4) That large individual differences in the range of cognition exist among our *O*s.

(5) That the individual differences vary inversely in size with the degree of cognition required for report.

The greatest differences in range among our *O*s occur in the experiment (Experiment II) requiring the lowest degree of cognition, and the smallest differences occur in the experiment (Experiment V) requiring the highest color-blind into 7 groups which are supposed to correspond to seven stages in the evolution of the color sense in the race. To the first group the entire spectrum appears gray. The second group sees a tinge of red at one end of the spectrum and of blue at the other. For the next group the colored sections invade the gray until they meet in the center, and are seen as blue and yellow. Next a third color, green, appears between the two. Such cases see red, green, and violet, but not blue and yellow. The next group see red, yellow, green and violet, and the sixth see red, yellow, green, blue and violet. Orange is the sixth color to be recognized. This is the normal group. The highest development yet reached is that of the group who can distinguish indigo in addition to the other colors. This is the sort of classification and description which we need. Unfortunately, few, if any, agree with this grouping.

(*C*) *Ishihara's classification.* Ishihara,[5] Japanese inventor of the test described later, accepts the red- and green-blindness classification, but finds that some cases of red-green blindness are accompanied by a weakness in the blue-yellow sense, while some are not. The former type of defect he designates as "incomplete total color-blindness," that is, a weakness for all colors.

(*D*) *Collins's results.* In a recent book, Collins[6] makes a study of ten cases of red-green blindness, in an attempt to find out more exactly, uninfluenced by any color theory, just what the color experiences of the color-blind really are. Most of the standard tests for color-blindness, as well as equations on the color wheel, contrast experiments, etc., were used.

Collins gives a detailed statement of the confusions made by each *S*. She found that the deuteranope and protanope classification held good, but was inadequate for a complete description, as each case was unique and each represented a different stage of defective vision. One *S* apparently saw a small band of blue on one side of the spectrum and a corresponding one of yellow on the other side with only two broad gray bands between, while others could actually see red and green under certain conditions. The final conclusion of Collins is that "it seems unprofitable to systematize the color sensations of the color-blind; so many individual types exist."[7]

(*E*) *The four-stage classification.* The author wishes to offer, as a result of her work with 60 color-blind *S*s, a new explanation of what the color-blind see, which takes account of the quantitative and qualitative differences found, and which offers a practical scoring method.

Most work in color-blindness has been done either with a few colors commonly confused by the color-blind, or with a great hodgepodge of all sorts of unstandardized colors, or with the hues of the spectrum in carefully standardized wave-lengths. All of these methods were felt to be inadequate. The color unit which is psychological in character is the color sphere, and a test which aims to find out the facts of color vision must take all three dimensions of the sphere—hue, saturation, and value—into account. By using a color-sorting test in which the colors were chosen to represent the sphere as completely as possible, the four-stage classification of the color-blind was arrived at. It may be described as follows.

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[5] S. Ishihara, *Tests for Colour-Blindness*, 5th ed., 1925.
[6] *Op. cit.*, 189 ff.
[7] Collins, *op. cit.*, 210.

If we had some way of finding out exactly what the color sphere looks like to each of our *S*s, and could make an objective representation of the spheres as they appear to each separate individual, we should find that these spheres represented a gradually descending scale of color vision from the almost perfect to the nearly monochromatic. Although we had none among our *S*s, there are individuals reported who see every hue of the sphere as neutral gray. If we tried, however, to arrange these color spheres, or objective representatives of what each *S* saw, in an orderly series such that the best was at the top and the poorest or most nearly colorless at the bottom of the scale, and such that the series varied only quantitatively (in amount and number of colors seen) and not qualitatively (some seeing a different assortment of colors than others), we should fail, because there is every variety of individual difference in color vision. But if, instead of using the whole color sphere, we should confine ourselves to a single hue, we would have no difficulty in forming such a series. The representations of what each individual saw, for instance, when he looked at the red cards of a test which are colored to represent many different shades, tints, and saturations of the red hue, could be readily arranged in a series such that at one end there would be the true red as seen by the normal eye, and at the other end the pure gray or black, as red appears to the worst cases. Furthermore, if such a series were made, it would probably be found that the color vision of the many *S*s for this one hue could be roughly divided into 4 classes. Those who saw red normally would be in stage 1, those who saw it as gray, in stage 4, and there would be at least two intermediary stages. Each hue of the spectrum could be classified for each *S* as being in one of these 4 stages. This was our notion as first formulated. As a matter of fact it does not hold in this form because, besides the quantitative difference described, there is also a qualitative one. As actually found in practice the facts seem to be as follows.

Stage 1. The hue is seen normally in Stage 1.

them they wrote the number of the sample it resembled in the square containing the number of the card. When the pack had been worked through in this way, display sheet No. 2 was used in place of No. 1 and the performance was repeated. The whole procedure was repeated a third time with display sheet No. 3. The pack was then exposed a fourth time while the *Ss* named the color on each card, using a list of abbreviations printed on a card placed before them. The whole proceeding took about 40 minutes.

III. RESULTS

(*a*) *The Four-Stage classification of the color-blind.* The results for each *S* on the color-sorting test were collected on a separate sheet, a sample of which is given as Table I.

The numbers in boldface in Table I are those of the 95 color-cards. The captions give the true color names of every group together with the number of the sample card under which the group should have been classified. Opposite the boldface numbers appears the name the *S* gave the exposed color-card and the number of the sample card under which he classified it. A dash(—) indicates that the card was left unclassified at that exposure. An asterisk (*) before a number indicates that it is identical with the sample card. The numbers in italic are the standard ones giving the saturated hue.. While Table I will not mean much at first to the reader, for those familiar with the meaning of every number and abbreviation it shows the *S*'s confusions at a glance.

In working over the papers the Four-Stage classification outlined above was formed, at first tentatively, and was tested by drawing up a list of rules and scoring in the light of them each *S*'s color vision for each hue. The rules, given below, are either self-evident propositions or are based on some of the most commonly accepted facts about color-blindness, such as the fact that these people see yellow and blue better than they do other hues.

(1) If all the cards of a certain hue are named and classified correctly throughout, it is taken as evidence that the *S* sees that color as the normal person does, and he is given a rating of '1' in that hue.

(2) The mistakes commonly made by the control *Ss* were tabulated and of course were not counted as having any significance.

(3) If a card is called gray or is classified under the gray sample it is evident that it is seen as gray. (See page 213 of Collins.) There were exceptions, however.

(4) If two or more hues are named or classified alike they appear as the same color.

(5) To confuse colors which lay adjacent to each other on the color ring was not counted as a significant mistake. The controls frequently made this error.

(6) If an *S* confuses two colors which both lie on the same side of the color ring, he sees them both as the central color of that side, that is, as either blue or yellow, unless further evidence proves that they are seen as gray.

(7) If two colors are confused which lie on opposite halves of the ring, they are both seen as neutral gray, unless the two colors are adjacent to each

other. Thus red and purple may be confused and still be seen normally, because they are neighbors, but if green and purple are confused, both are seen as gray.

(8) If a wide variety of color names is given in a perfectly random fashion to the cards of one hue, that hue is seen as a neutral gray.

(9) If the gray cards are generally called by a color name, there is a neutral band in that color.

(10) If colors are called blue, yellow, or gray, they are probably seen as such, unless it can be shown that the *S* has a neutral band in the blue, because these are the hues commonly seen correctly by the color-blind.

TABLE I

The Results of *S* 34, Collected and Arranged for Grading

The numbers in boldface are those of the color-cards. The captions give the color of every group and the number of the sample card under which every member of the group should have been classified. Opposite the boldface numbers appear the names given the exposed color cards and the numbers of the sample card under which each was classified. A dash (—) indicates that the card was left unclassified; an asterisk (*) indicates that it is identical with the sample card; and the boldface italic indicates the cards of standard hue.

g = green, gr = gray, b = blue, bk = black.

Brown 1					*Red 7*										*Orange 3*				
__6__	g	—	—	10	__12__	gr	—	—	10	__8__	—	—	—	—	__17__	y	4	—	—
__33__	br	1	—	10	__39__	gr	—	—	10	__20__	gr	2	—	—	__35__	y	3	—	—
__46__	br	1	7	10	__48__	g	1	—	10	__25__	—	1	—	—	*___44___	y	4	—	—
__61__	br	1	7	—	__55__	g	2	7	—	__31__	gr	1	—	10	__77__	o	3	7	—
___75___	br	1	7	—	___59___	—	—	7	10	__49__	—	1	—	—	___80___	g	1	7	10
					__72__	br	1	7	10	__66__	gr	—	—	10	__3__	y	—	—	—
					__89__	g	1	7	10	__87__	g	1	7	10	__63__	g	1	7	—
					*__93__	g	1	7	10						*__83__	g	2	7	10

Yellow 4					*Green 10*										*Gray 2*				
__4__	y	4	—	—	__19__	gr	—	—	—	__26__	br	—	—	—	__2__	w	—	—	—
__16__	y	4	—	—	*__32__	—	—	—	10	__47__	g	1	—	10	__15__	b	—	6	9
*___36___	y	4	—	—	__43__	—	—	—	—	__69__	g	2	6	10	__37__	r	2	6	8
__64__	y	3	—	—	__54__	yg	2	7	10	__73__	—	1	7	10	__41__	pk	5	6	8
__40__	r	3	—	—	___60___	gr	—	—	10	__85__	—	2	—	10	*__62__	pk	2	—	8
__78__	o	—	—	—	___67___	g	1	7	10	__94__	g	—	7	—	__79__	r	2	6	8
___81___	g	1	7	10	__7__	gr	—	—	—						__82__	b	—	—	—
					__13__	b	4	—	—						__95__	bk	—	—	—

Blue-Green 8					*Blue 9*					*Violet 6*					*Purple 5*				
__21__	pk	—	—	8	__22__	b	—	—	9	__23__	b	—	—	9	__1__	bgr	—	—	10
*___74___	b	—	5	8	__51__	b	—	—	9	__28__	b	—	—	9	__5__	p	—	5	—
__58__	pk	2	6	8	*___57___	b	—	—	9	__52__	b	—	—	9	__53__	b	—	—	—
___70___	pk	2	—	8	__71__	b	—	—	9	___68___	b	—	—	9	__65__	b	—	—	9
__9__	b	—	—	9	___90___	bk	—	—	9	__11__	b	—	5	6	___76___	v	—	5	9
__14__	v	2	6	8	__10__	v	5	6	9	*__56__	pk	—	8	—	__18__	b	—	6	9
__24__	r	—	6	8	__29__	v	5	—	8	__91__	v	2	5	8	__27__	p	2	5	6
__30__	pk	2	6	8	__92__	pk	5	—	8						__34__	p	2	—	8
__38__	v	—	5	—											__42__	p	—	6	8
__50__	r	2	6	8											*__45__	p	—	6	8
__88__	b	—	—	9											__84__	b	—	—	—
															__86__	b	—	—	—

(11) If the cards of two hues are both classified under the the same two or three sample cards consistently, they are seen alike, though they may be named differently. When the inconsistency is pointed out, the *S* will say in an aggrieved tone, "Well, I can only tell you how it looks to me."

(12) If both the light blues and the light violets are called blue, and the dark members of the two series are called violet, they are both seen as blue. The explanation is that they are being judged on the basis of brightness and not hue. So for any other hues which are treated in the same way.

(13) The fact that an *S* has classified all the cards of a given hue correctly does not count in his favor, if he has in addition made many other false classifications of them.

(14) The fact that a card is not classified under the blue or yellow is not a certain proof that it is not seen as one of these two colors. An *S* may classify each of the blue-green, violet, and purple cards under all the 3 sample cards of those hues; according to rule (5) above, they are all seen as blue. Yet they may not be included in the cards classified under the blue sample card, and all the blue cards may be correctly classified and named. The explanation is that they are seen as blue, but as a "different color blue," as the *S*s describe it. They appear as a "dead-looking" blue because the red and green in them appear as gray, and they may thus be readily distinguished from the clear bright blues. This is further proven by the fact that the unsaturated and dark blues may be classified with the blue-green, violet, and purple series, rather than with the blue cards.

(15) A color may be named correctly and classified incorrectly. If this is done systematically, the color is seen as classified rather than as named.

While this method of grading the papers is not purely objective and requires a certain amount of 'clinical sense,' it was not found to be as difficult as one would imagine to assign to each *S* a rating for each hue of his color ring which was felt to represent the facts pretty clearly.

Table II gives the ratings according to the Four-Stage classification. It will be seen that in some cases the ratings of 2.5 or 3.5 were used. This was done when the fairest rating seemed to be between 2 and 3, or 3 and 4.

Unexpected confirmation of the theory suggested was offered by one of the *S*s. He claimed to have had normal vision up until four years ago when he had scarlet fever which left him with a curious defect. In foveal vision he exhibits all the ear marks of a typical case of dichromatism; in peripheral vision he has no difficulty in distinguishing red and green. An examination of his eyes with the perimeter showed in each a ring which was sensitive to these colors, but which enclosed an area covering the fovea which was sensitive only to blue and yellow. Here we had an individual capable of describing the sensations of the color-blind in terms of normal vision. His results on the test bear striking witness to the truth of the theory advanced above.

(*b*) *Explanation of types of confusions made.* One of the most puzzling facts brought to light by the color-sorting test was that such different types of confusions were made by different *S*s. Of two *S*s apparently equally color-blind according to the Still-

TABLE II

Ratings Received by the Color-Blind According to the Four-Stage Classification Arranged According to Rank Order of Scores

S	Yellow				Blue				Total Score
	R	O	Y	G	BG	B	V	P	
1	1	1	1	1	1	1	2	1	9
2	1	1	1	2	1	1	1	1	9
3	1	1	1	2	1	1	1	1	9
4	1	1	1	2	1	1	1	2	10
5	1	1	1	2	gr2	1	2	1	11
6	1	1	1	2gr	gr3	1	1	1	11
7	1	1	1	2	gr2	1	2	1	11
8	gr2	1	1	2	gr2	1	1	1	11
9	1	1	1	2	y2	2	1	1	11
10	3	1	1	1	gr1	3	1	1	12
11	2	1	2	2	1	1	2	2	13
12	gr2	1	1	2	1	2	2	2	13
13	gr2	1	1	3.5	gr3.5	1	1	1	14
14	2	1	1	3.5	gr3.5	1	1	1	14
15	2	2	1	3gr	gr2	1	2	2	15
16	3	2	2	2gr	1	1	2	2	15
17	2	1	1	2gr	4	1	2	2	15
18	2	2	1	2	gr3	2	2	1	15
19	2	2	1	3	gr3	1	2	2	16
20	2	1	1	3	y3	2	2	r2	16
21	1	2	1	3	4	2	2	1	16
22	2	2	1	2gr	4	2	2	2	17
23	3	1	1	4	4	1	2	1	17
24	2.5	1	1	4	4	1	2.5	2	18
25	2	3	1	3	y3	1	3	r2	18
26	2	2	1	3	4	1	2	4	19
27	2	2	2	3	y3	2	3	2	19
28	gr2	2	1	3	4	1	3	3gr	19
29	3	1	2	3	4	1	3	3	20
30	4	1	1	3	4	1	3	3	20
31	2.5	2	2	2.5	4	3	2	2	20
32	3	2	1	3	b3	2	3	3	20
33	3	3	1	3	4	1	3	3	21
34	3	2	2	3	4	2	2.5	3	21.5
35	4	2	1	3.5	4	2	2	3	21.5
36	2	3	1	3	4	3	3	3	22
37	3	3	1	3	4	3	2	3	22
38	4	2	2	3.5	4	2	2	3	22.5
39	3.5	2	2	3.5	gr3	2	3	4	23
40	3.5	2.5	1	3.5	4	2	3	3.5	23
41	3	2	2	4	4	3	3	2	23
42	3	2	2	4	4	3	3	r2	23
43	4	3	2	3	4	3	3	2	24
44	4	3	1	3	4	3	3	3	24
45	3.5	3	1	4	4	2	3	4	24.5
46	4	3	2	4	4	2	3	3	25
47	3.5	3	3	4	4	2	3	4	26.5
48	3.5	3	1	4	4	3.5	3.5	4	26.5
49	4	4	2	3	4	3	3	4	27
50	4	4	2	4	4	4	2	4	28
51	4	3	1	3	4	4	4	3	30

ing Tables, one would consistently confuse the red and green cards, while the other would not make one such confusion. On the other hand, the latter would confuse red with brown and yellow, and green with gray, so consistently that one could well doubt whether he saw either red or green at all. In other cases *Ss* confused the unsaturated reds and greens, but saw the bright ones well enough to identify them correctly.

The explanation of these and similar phenomena becomes clear in the light of the Four-Stage classification of the color-blind. It is self-evident that those cards which looked alike were confused while those which appeared different were distinguished from each other.

(1) If both red and green are in Stage 1, no confusion can take place.

(2) If both hues are in Stage 2, the brightest colors which have the normal hues will not be confused, but the pale, dark and unsaturated ones will both be seen as yellow or blue and may be confused.

(3) If two colors are in Stage 3 so that the bright, saturated colors are seen as yellow, and the others as gray, confusions will be found to exist between all the reds and greens.

(4) If both are in Stage 4 confusion will be complete.

(5) If, on the other hand, red is in Stage 1 and is thus seen normally, and green is in Stage 2 and seen as green and yellow (or vice versa), no confusion will take place.

(6) If red is in Stage 1 and green in Stages 3 or 4 (or vice versa), being seen as yellow and gray, or pure gray, there will be no confusion because the color seen normally can always be distinguished.

(7) If one of the hues is in Stage 2 and the other in Stage 3, there will be some confusion between the shades and tints which are yellow, but not very much.

(8) If one is in Stage 2 and the other in Stage 4, there will be no confusion, since one is always colored and the other appears gray.

(9) If one is in Stage 3 and the other in Stage 4, confusion will take place between the color which appears gray in Stage 3 and that which appears gray in Stage 4.

(10) In cases where the green is not changed to yellow, but simply becomes a less saturated green, while the red is seen as yellow, there will be no confusions except when one is in Stage 3 and the other in Stage 4.

(11) Colors from opposite halves of the circle will not be confused unless both are in Stage 3 or 4.

These rules work out consistently, not only for the reds and the greens, but also for the hues which occupy the corresponding positions on the other half of the ring, namely, purple and blue-green.

(c) *The color ring.* From the vertical columns in Table II the average rating given the *Ss* for each hue was computed. When these figures are placed in the color ring, several interesting points are evident (see Fig. 1). There are four points of importance on the ring; these have been labeled A, B, C, D. The ratings indicate that there are two points, A and C (the blue-green and red), at which difficulty with color vision starts. From these two points difficulty spreads in both directions, but as we progress from points C and A to points B and D, color vision becomes progressively better. At B and D it is best. The whole thing is not symmetrical, however, for B is a point of much clearer vision than D. Blue-green is likewise (point A) more often confused than red. The numbers outside the smaller circle indicate the average grade of color vision for each hue: blue-green being most often confused, then green, red, purple, violet, orange and yellow in the order named.

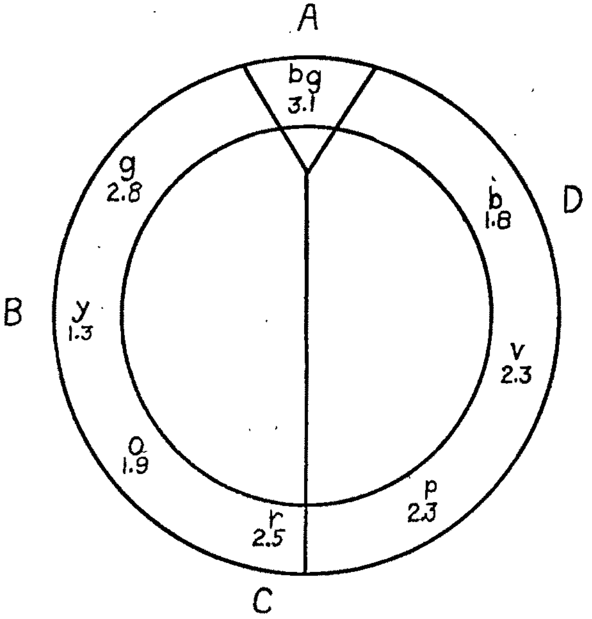

FIG. 1 THE COLOR RING

These numbers, however, are derived from averages. Do all individual cases consistently follow this order? A study of Table II shows that they do, for almost every case is weakest at A, tests disagree very seriously in selecting the 'totally green blind' and the 'totally red blind.'

The Ishihara test uses a purple number on a gray background to select the green-blind. Evidently the theory is that the totally green-blind always have a neutral band in the purple. According to the color-sorting test, this is not true. There are 9 *S*s who are rated a 4 in the green. Of these only 4 show evidence of seeing purple as gray also. The Ishihara test was successful in picking out all of these four. Two *S*s have a gray band in the green according to the sorting test, but are not classed as green-blind on the other test. What is more serious, 14 out of 22 *S*s classed as green-blind on the Ishihara test showed no evidence of being completely blind to the green. *S* 10 is even rated as 1 in the green.

If we assume that these cards with their purple number on the gray background fail to select the green-blind, but pick out those with a neutral band in the purple, we are no better off. Of 22 *S*s who failed to read the numbers, only 6 clearly saw gray instead of purple. If we include those whose purple is in Stage 3, who see some purple as blue, the number is raised to 11.

Numerous other cases could be cited. How can we explain the fact that an *S* can identify, name, and classify correctly nearly all the red cards in a pack, and yet cannot read a bright red number on a gray background which is as clear as day to the normal eye? And how explain the fact that some *S*s failing to read the number could actually name the colors of the dots?

W. R. Miles suggests that the area on the retina capable of receiving the sensations of red and green may in such cases be very much shrunken from the normal. In that case an *S* could see a spot of color, or could identify the hue of a large area of color, but would be unable to see all of a 2-in. number at once, for most of it would appear as uncolored. This hypothesis is substantiated by the fact that some *S*s were found who could not read the figures except at a distance of several feet. Also one of the *S*s who could sort colors but could not read the figures was tested with a perimeter; her field for the red and green was found to be shrunken to a tiny area around the fovea. Another possibility would be that the part of the retina sensitive to red and green had been broken up into irregular sections, some color-sensitive and some color-blind, so that the whole number would not be registered.

Another explanation is that the number cards use, for the most part, somewhat pale, unsaturated colors, rather than standard spectral ones. We have shown that changes in value and saturation are highly important. Because an *S* fails to read a pink letter on a pale green background, we cannot therefore conclude that he will always confuse red and green. Similarly, because he does not see a grayed purple number on a gray background, we cannot leap to the conclusion that he has a neutral band in that hue.

Whether the explanation here offered is correct or not, it would certainly not be right to discard the number tests on the ground that they do not give the same results as the color-sorting tests. The difference is due to the fact that two quite distinct situations are presented to *S*, in one case a field of unbroken color, in the other small dots of different colors. The latter situation is far more often met with in real life than the former. In looking at a picture or a landscape, color is seen in small broken areas, rather than in large unbroken ones. The numbers cannot be neglected in testing color vision, for they offer a unique contribution.

IV. FUTURE WORK

A more objective method of testing the truth of the Four-Stage classification is desired. A possible technique would be to use number cards with a wider variety of colors. Four or five pairs of complementary hues would be necessary. Six of each hue, in three values, with two saturations of each value, would be necessary. The warm hues would then be used to paint numbers on backgrounds of appropriate grays and yellows,the cold numbers on backgrounds of blues and grays. Blue and yellow combinations would also be necessary. A hundred such cards would form a thorough, rapid, and objective means of testing out the Four-Stage classification described above.

V. SUMMARY

(1) The color sphere rather than the spectrum should be used as a basis for tests of color-blindness, since differences in saturations and brightness are for the color-blind differences in hue.

(2) An individual's color vision may be described in terms of the Four-Stage classification of color vision. Each hue is given a rating of 1, 2, 3, or 4. A rating of 1 indicates perfect vision in that hue, a 4, that the hue is seen as gray. A 2 means that the bright saturated colors of the hue are seen in their true hue, though somewhat less saturated than they appear to the normal eye, while the light, darker, and less saturated colors are seen as the central unit color of the side of the spectrum on which they lie; that is, as yellow or blue. In Stage 3 the saturated colors are seen as either yellow or blue and the others as gray. Occasionally a color does not change in hue, but is seen merely as a weaker saturation of the true hue.

(3) The many different types of confusions made by different color-blind individuals are explained by this classification, for only colors which are seen alike will be confused.

(4) On the color ring there are four critical points. Color vision is poorest at blue-green, grows progressively better as we proceed to yellow, grows worse again to red, better as far as blue, and worse again till blue-green is reached.

Treatment of Data. Since equal numbers of learning trials have been given for rest and work and since we are comparing throughout records on the same group of *Ss*, it is possible to compare directly the retention scores under the two conditions. The methods of scoring the retention and of finding the amounts of retroactive inhibition are as follows.

(1) The total number of syllables correctly anticipated at the first relearning presentation has been found for each degree of learning under each condition. These scores will be referred to as recall scores. The difference between the recall scores for the rest and work conditions represents the absolute amount of retroactive inhibition at each degree of learning.

(2) Inasmuch as it is more important, however, to consider in our measure of retroaction the amounts of the scores between which differences obtain, the relative amount of retroaction is the significant score. This is represented by the percent which the difference between the rest and work conditions is of the score under the rest condition. The rest condition is obviously the standard against which the work results must be judged.

(3) The total number of trials required for relearning to one perfect repetition has been found for each condition and each degree of learning. The difference between the rest and work scores gives the absolute amount of retroaction, at each degree of learning, in terms of relearning trials required.

(4) Saving scores have been computed from the data of (3) by the formula:

$$\text{Saving Score} = 100 - \left(\frac{\text{No. Presentations Required to Relearn} - 1}{\text{No. Learning Presentations}} \times 100.\right)$$

The −1 in the numerator is to deduct the last and perfect repetition. It is absent in the denominator, because the number of presentations on any experimental day is fixed and, with the smaller numbers of repetitions, is not always sufficient for complete mastery. The absolute amount of retroaction is, of course, the difference between the rest and work saving scores.

(5) Relative retroaction in terms of saving has been found by subtracting the work from the rest scores and dividing by the rest score.

(6) In order to find the degree of learning, as an auxiliary measure to the number of presentations given, an overlearning ratio has been computed for each list by the formula: $\text{Overlearning Ratio} = \frac{\text{Anticipations}}{\text{Failures to Anticipate}} \times 100.$

RESULTS

The total recall scores appear in Table I. Each figure in this and in all later tables has been obtained from 20 individual records.

TABLE I

SYLLABLES CORRECTLY ANTICIPATED AT FIRST RELEARNING PRESENTATION

	Number of Repetitions in Learning				
	6	11	16	21	26
Rest	73	102	108	143	134
Work	13	22	38	55	72
Difference	60	80	70	88	62
D/R	82.2%	78.4%	64.8%	61.5%	46.2%

The total recall scores increase with increasing numbers of learning presentations, save from 21 to 26 repetitions under the rest condition. The increase is more regular under the work than under the rest condition. The differences between the scores under the two conditions show an irregular relation between number of presentations and gross retroaction. When, however, the relative amount of inhibition is considered, the relation becomes much more regular. The relative inhibition for each cycle of the experiment and for the two cycles combined is shown in Fig. 1.

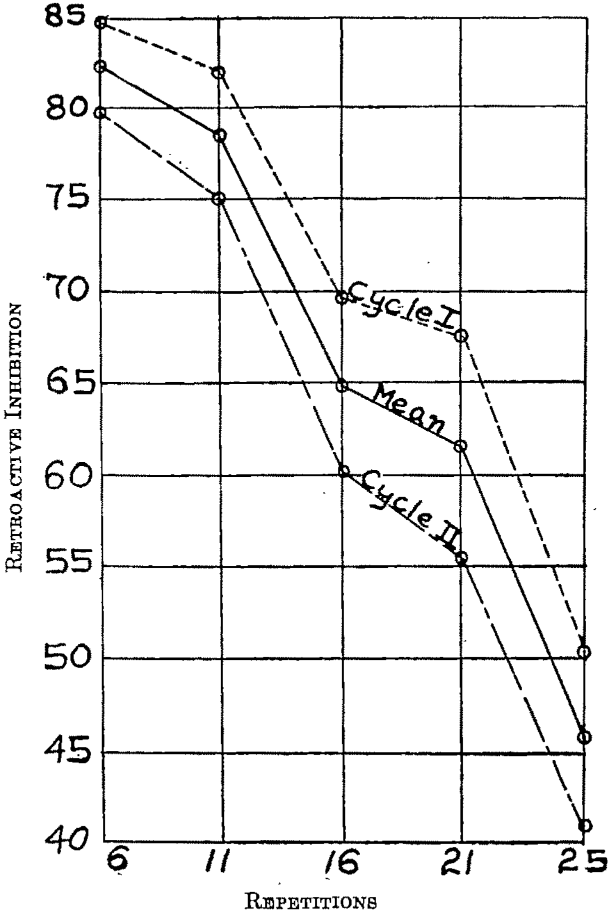

FIG. 1. RELATIVE RETROACTIVE INHIBITION RECALL SCORE

When only 6 repetitions are given the material to be learned, and when 11 repetitions of a second list are interpolated between the learning and recall of the first list, not only is nothing saved, but a slight loss occurs. The relative inhibition is, therefore, more than 100%. The difference between 6 and 11 repetitions, in terms of relative retroaction, is 85%, giving a very sharp drop in the curve. From 11 repetitions the curve falls slowly and regularly to a relative inhibition of about 5% at 26 repetitions.

The curves for the individual cycles are so close together that they are difficult to distinguish, when drawn. Only the composite curve is given, therefore, in Fig. 2. Cycle II shows the greater inhibition at the first three points and the less inhibition at the remaining two points. Thus, no consistent relation appears between practice and degree of retroaction.

The conclusion is clear that, measured in terms of saving score, retroactive inhibition varies inversely as the number of presentations given the material to be learned. The curve of inhibition plotted against number of learning repetitions shows marked negative acceleration.

Corroborative evidence of the validity of the conclusions drawn is given by the fact that the two *S*s who were trained both in the use of the anticipation method and in general psychological experimentation, and who reported practically perfect rest periods throughout the experiment, give curves for both recall and saving retroaction which are closely similar to those of the eight untrained subjects.

It is possible that increase in the number of repetitions given the nonsense syllable lists might not have meant a corresponding increase in the degree of learning. Consequently, the overlearning ratios for each *S* have been computed. The mean ratios for the rest and work conditions, and for the interpolated learning, are shown in Fig. 3. The rest and work overlearning ratios increase as the number of repetitions, with a slight negative acceleration. Clearly, increase in the number of presentations represented a corresponding increase in the degree of learning—or of overlearning. There is no apparent reason for the small but uniform difference between the ratios under the two conditions. The lower ratios of the work condition may have served to accentuate the degree of retroaction, but it can hardly have affected the tendency of the results.

The rise in the curve for interpolated learning from 6 to 21 repetitions, small though it was, was hardly to have been expected. One would expect fluctuations, but not regular increases. It is unlikely that it is the result of practice, since all positions on the curve had equal average position in the experiment. The regularity is not likely to be the result of chance factors. It may be the

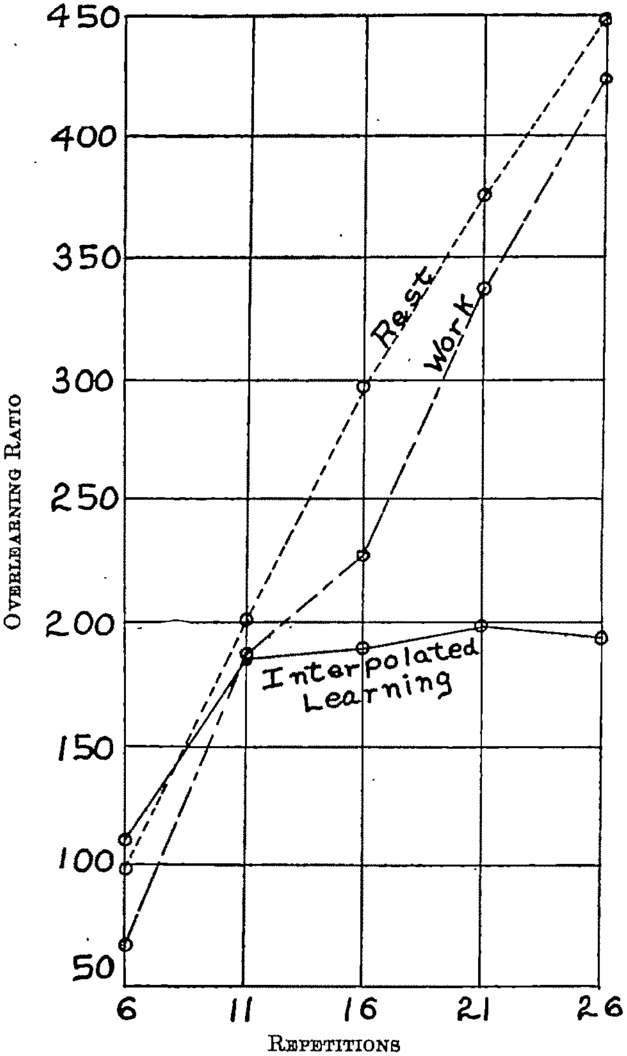

FIG. 3. MEAN OVERLEARNING RATIOS

and saving methods. This is true of both rest and work conditions, with but one slight exception.

(3) The relative amount of retroactive inhibition, when retention is measured by the recall method, varies inversely as the number of presentations given the material to be learned. It is particularly significant that a list learned even to 26 repetitions, with correspondingly high overlearning ratio, is unable to resist the disintegrating influence of retroactive inhibition from a list of equal length learned to 11 repetitions. With the 26 repetitions there is still a disintegration of 46.2%. Practice tends somewhat to reduce the susceptibility to retroaction as shown by recall. The absolute amount of retroaction, measured by the difference between rest and work scores, shows no constant relation to degree of learning.

(4) The relative amount of retroaction, when retention is measured by the saving method, varies inversely as the number of learning presentations. The curve of inhibition plotted against the number of repetitions for learning shows marked negative acceleration, and falls from 108.5% at 6 presentations to 5.3% at 26 presentations. The absolute differences between rest and work scores for relearning and saving tend also to decrease with increasing learning.

RELIABILITY OF THE GALVANIC REFLEX*

By ALVHH R. LAUER, Ohio State University

Since the first use of the galvanic reflex for measuring emotional reactions by Féré[1] in 1888, many investigators have studied the phenomena. Various measurements of bodily changes, memory, effects of alcohol, atropin, and other drugs on the organism, anaesthesia, and sleep have been made with the galvanometer. Further, it has been used as a complex indicator and in clinical diagnosis of mental disorders. Other uses of the galvanic reflex have been suggested and investigated to some extent; notably in the experimental studies of Marston on emotional reactions, Darrow[2] on secretory changes in the skin, and Wechsler,[3] who studied general correlated effects. Without commenting on the merits of these experiments or the nature of the reflex, it suffices to say that only in a general way has any standardized technique been employed, and usually results are not comparable. For the purpose of convenience we shall adopt conventional terminology and speak of changes in potential as changes in body resistance, although this practice has been shown to be seriously at fault by Gildemeister[4] and discussed at some length by Prideaux.[5]

Problem. While working on the major problem of investigating bodily changes concomitant with the galvanic reflex, the writer made a careful search of the literature for a satisfactory technique. Different methods have been used with some degree of success but little has been said concerning the reliability of results obtained.

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[4] M. Gildemeister, Der sogenannte psycho-galvanische Reflex und seine physikalisch-chemische Deutung, *Arch. f. d. ges. Physiol.*, 162, 1915, 489-506.

[5] E. Prideaux, The psychogalvanic reflex: a review, *Brain*, 43, 1920, 50-73.

were held relatively constant, the next step was to calibrate the instrument and determine the significance of a given deflection with a given initial resistance. To do this a complete set of resistances was procured, and an electrical engineer aided in calibrating the apparatus. Various known resistances were substituted for *S*, and the apparatus was calibrated from these. It was found that when the resistance in the bridge was held constant the deflections of the galvanometer gave a linear relationship with the change in resistance (see Fig. 1). This was merely a test of the accuracy of the galvanometer, since it was constructed to give this relationship. When the resistance in the bridge was changed, however, this function was altered, as may be noted from Fig. 2.

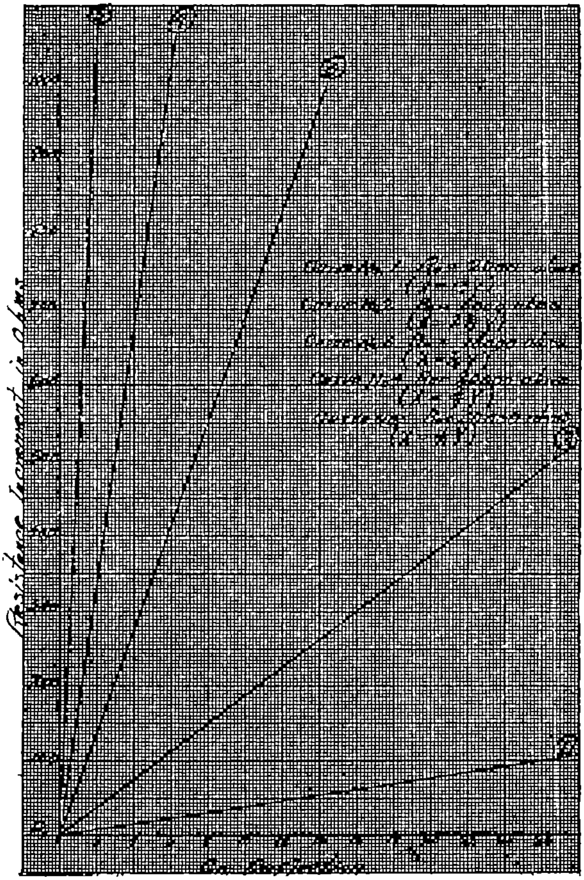

Fig. 1. Deflections of Galvanometer with the Change in Resistance

The data were now studied and self-correlations between significant aspects of the measure were calculated. According to Garrett[13] the index of reliability is the highest possible correlation which could be expected between obtained scores on a given test and the true scores on the test. In all cases where self-correlation was used for establishing the reliability of a measure these values also have been calculated. The means, sigmas, and coefficients of variability have also been computed to aid in properly evaluating the data.

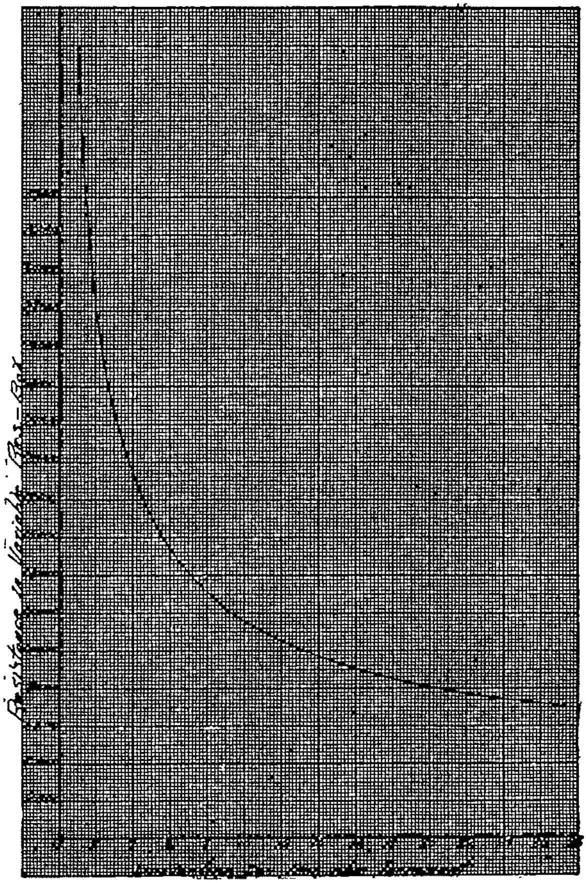

FIG. 2. DEFLECTIONS OF GALVANOMETER WITH CHANGES IN THE RESISTANCE OF THE BRIDGE

The order followed in the treatment of results has been as follows: (1) to determine gross reliability of the total 33 stimuli (Table I) obtained from 22

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[13] H. E. Garrett, *Statistics in Psychology and Education*, 1926, 273.

Ss on 348 reactions; (2) to find the reliability of 4 specific stimuli used in a study of the validity of the galvanic reflex taken alone and then combined (Table II); (3) to study the reliability of the reflex on one *S*, using several stimuli in repeated reactions (Table III); and (4) to find the correlations of various related factors involved (Table IV).

TABLE I

Reliability Coefficients from 348 Reactions of 23 *Ss* Using 55 Different Stimuli

r = raw coefficient of correlation on duplicate test; R = index of reliability; M_1 = mean of first measure; M_2 = mean of second measure; $S.D._1$ = standard deviation of first measure; $S.D._2$ = standard deviation of second measure; C_1 = coefficient of variability of first measure; C_2 = coefficient of variability of second measure.

Measure	r*	R	M_1	$S.D._1$	C_1	M_2	$S.D._2$	C_2
Deflections	0.6188	0.78	8.16	5.66	0.69	7.72	6.15	0.79
Ohmic change	0.5223	0.72	1501.00	971.00	0.64	1342.00	1191.00	0.88
% change	0.5528	0.74	8.54	3.78	0.67	8.63	5.98	0.69
Mean	0.5653	0.748			0.67			0.79

*Correlations have been carried to four places to insure accuracy in calculating multiple Rs by the method of Tolley and Ezekiel.

TABLE II

Reliability of General Stimuli Used with 3 *Ss*
(For explanation of captions see Table I)

S	Reactions	r	R	M_1	$S.D._1$	C_1	M_2	$S.D._2$	C_2
O	37	0.6420	0.80	6.83	8.27	1.2	7.00	7.32	1.1
S	22	0.3602	0.60	10.23	7.76	0.75	6.46	7.61	1.1
F	31	0.6474	0.81	9.13	7.58	0.83	4.91	4.91	1.0
Mean		0.5449	0.73			0.93			1.06

TABLE III

Correlations of Factors

Initial resistance and deflection	−0.0924	±0.03
Initial resistance and ohmic change	+0.3322	±0.03
Initial resistance and percentage change	+0.0071	±0.03
Self-correlation of initial resistance	+0.8843	±0.01
Percentage change and deflection	+0.8843	±0.01
Deflection and percentage change	+0.8990	±0.01

TABLE IV

Comparison of First and Second Reactions
(For explanation of captions see Table I)

Measure	M_1	$S.D._1$	C_1	M_2	$S.D._2$	C_2	Diff. in Means	P.E. Diff.	Ratio
Initial ohms resistance	17691	13295	0.75	17883	12720	0.71	192.0	941.00	0.21
Deflections	8.16	7.72	0.94	5.66	6.15	1.10	2.5	0.49	5.10
Ohmic change	1501.00	1342.00	0.89	971.00	1191.00	1.20	530.0	0.92	5.76
% change	8.54	8.63	1.00	5.78	5.98	1.00	2.7	0.54	5.10

TABLE V

SUMMARY OF RESULTS ON SPECIFIC STIMULI
(For explanation of captions see Table I)

Stimuli	r	R	C_1	C_2	C_m
Bell	0.6904	0.83	0.84	0.92	0.88
Skull	0.7774	0.88	0.99	1.08	1.03
Whistle	0.8169	0.90	1.16	1.20	1.18
Rifle	0.5701	0.75	0.98	0.79	0.84
Mean	0.7137	0.84	0.97	0.99	0.98

TABLE VI

RELIABILITY OF MEASURES IN ORDER OF MERIT
(For explanation of captions see Table I)

Measure	r	R	C_1	C_2	C_m	Ratio of P.E. of Diff. to Diff. in M_1 and M_2
% change	0.7012	0.84	0.85	0.83	0.84	5.11
Deflections	0.6593	0.81	0.91	1.00	0.96	5.10
Ohmic change	0.7104	0.84	1.02	1.04	1.03	5.76
Initial resistance	0.8843	0.94	0.75	0.71	0.73	0.21

Conclusions. (1) The percentage change or ratio of apparent ohmic change to initial bodily resistance seems to be the best measure of the galvanic response to sensory stimuli for the following reasons. (a) It is more reliable than deflections, with which it correlates +0.899. (b) The initial resistance has no effect on this measure, since the correlation was +0.007. (c) It is less variable than either of the other measures and hence would have a higher predictive power, other things being equal. (d) The variability of the first and second tests are more nearly equal. (e) It is more nearly independent of other influences measured.

(2) The relation between deflections and initial resistance is −0.092, or slightly negative. Smith[14] found a negative correlation between these two variables of −0.497. The present study would indicate that this negative relationship is a function of the apparatus and the resistance of the subjects used. The high variability of absolute deflections makes them less valuable as a unit of measurement.

(3) Ohmic change shows rather higher reliability than do deflections, but it is not desirable as a measure for two reasons. (a) It correlates with initial body resistance +0.332, and a spurious self-correlation is obtained since initial resistance has a reliability of +0.94. (b) It is quite variable and hence not good as a predictive measure.

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[14] *Op. cit.*

(4) Generally, the galvanic reflex shows great variability since the instrument is delicate.

(5) The initial resistance could not be considered as a measurement since it is taken before the stimulus is given. It would not be wise even to use it as a factor of any weight. The use of percentage change allows for influence due to large differences in initial resistance.

(6) When all measurements are included there is considerable difference in variability between first and second tests. The reactions are smaller on the second test, probably due to mental set. For valid results it seems that first reactions are best. Moderately intense stimuli are most reliable.

(7) The reliability of the galvanic reflex is quite high and apparently higher than that of pencil and paper emotional tests now on the market. The validity must be thoroughly established in order to make the galvanometer an asset to psychological or physiological method.

(8) A possible increase in reliability could perhaps be obtained by using a formula for ascertaining the best weights and then combining different aspects of the measure, but the absence of data on the validity of the galvanic reflex makes this impracticable at the present time.

UNCONDITIONED SALIVARY RESPONSES IN MAN

By A. L. Winsor and T. L. Bayne, Jr., Cornell University

In man the quantity of saliva secreted by the parotid glands may show great variation in any given period.[1] This variability is due to the numerous sources of excitation and inhibition, either direct or indirect, conditioned or unconditioned. Any study of the conditioned responses involves necessarily an exploration of the unconditioned responses, in order that the latter may be controlled or given consideration. In our preliminary research in this field we noted what appeared to be other sources of direct stimulation in addition to the mechanical, chemical, and protopathic factors accounted for by Lashley. When all other sources of excitation were accounted for, the muscles of mastication and swallowing seemed to cause measurable disturbances. The purpose of this article is to report some of the data from our studies which would seem to indicate proprioceptive stimulation of the parotid glands.

The obvious difficulty in a study of this sort is to single out one muscle or group of muscles for action while related muscles are kept under control. It seemed impossible to solve this problem, but by training the *S* to cause certain groups of muscles to be more active than others for a designated period, significant tendencies may be observed. The muscles selected for observation were the masseter, which lifts the lower jaw, the temporal, which lifts the lower jaw and draws it somewhat backward, and the pterygoid, which draws the mandibula forward and to one side. These constitute with those of the tongue the chief muscles of mastication. The results of some of these attempts are illustrated in the following tables.

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[1] Cf. N. Krasnogorski, Ueber die Wirkungen mechanischer und chemischer Reizungen verschiedener Teile der Mundhöhle auf die Tätigkeit der Speicheldrüsen bei Kindern, *Jahrb. f. Kinderheilkunde*, 114, 1926, 268-276; I. P. Pavlov, *Conditioned reflexes: an investigation of the physiological activity of the cerebral cortex*, tr. by G. V. Anrep, 1927, pp. 430; A. L. Winsor, Conditions affecting human parotid secretion, *J. Exper. Psychol.*, 11, 1928, 355-363.

does not stimulate the glands directly, the reaction from swallowing 2 cc. of water every 5 sec. was compared with the reaction when 4 cc. were swallowed at the same interval. It was assumed that in the latter case a greater tension of these muscles would be involved. At least the excitatory effect of the new situation is doubled. The effect of the expansion of the stomach as a result of drinking so much water probably accounts for the extra secretion shown in Condition 7 and 8.[3]

TABLE II

EFFECT OF SWALLOWING ON PAROTID SECRETION

	Successive 5-min. periods and conditions of stimulation	Secretions in cc.
1.	Erect sitting position, mouth closed	0.24
2.	Swallowing movements every 5 sec.	0.68
3.	Repeated 1	0.27
4.	Mouth full of water, no muscular activity	0.30
5.	Swallowing 2 cc. of water every 5 sec.	0.43
6.	Swallowing 4 cc. of water every 5 sec.	0.93
7.	Water in mouth as in 6, but no swallowing	0.51
8.	Repeated A	0.46
9.	Repeated 1	0.22

Other possible explanations than that of proprioceptive stimulation of these glands need to be considered. One of the first to offer itself is the possibility that all of these reactions are conditioned rather than unconditioned responses. Our experience in studying conditioned salivary responses in man has taught us that any muscular activity involved in the analysis, preparation, or handling of food may become a conditioned stimulus. Even the muscles of the arm become a potential part of the total situation. Hence criteria should be applied to eliminate this possibility, if such can be found.

In the relation between stimulus and reaction there is supposed to be a marked difference between unconditioned and conditioned reactions. The appearance or absence of the unconditioned response is determined mainly by the strength of the stimulus. As the stimulus increases the response increases. In the conditioned reactions on the other hand a weak stimulus may be effective while a stronger stimulus invokes no response whatsoever. A review of the data presented will show a consistent relationship between increase of stimulus and increase in response. The greater the tension of the muscle, the greater the flow of saliva.

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[3] In a previous report a full stomach was shown to increase the secretion 100%. A. L. Winsor, *op. cit.*, *J. Exper. Psychol.*, 11, 1928, 355-363.

Furthermore, conditioned salivary responses as far as our experiments are concerned seem to depend largely upon the state of hunger of the *S*. When the organism is in a state of complacency as far as food is concerned the conditioned salivary responses become very weak or disappear entirely. This is not so true of the reactions under discussion. They are more stable and regular. In fact they have all the characteristics of reflex action. Fatigue interferes only when the stimulating muscle shows fatigue. In a 12-hr. test of the glands no evidence of fatigue was shown, although they secreted many times their weight in saliva. The conditioned response, on the other hand, soon disappears when not reënforced. Even so well established a response as the flow of saliva to the raising of the hand with food in a spoon was temporarily extinguished in one *S* after 45 min. of constant stimulation without reënforcement.

While there appears to be no similar mechanism for comparison, these reactions seem to resemble most the muscular contraction response evoked when the knee extensor is stretched. The receptors for this reflex lie in the fleshy region of the muscle and constitute it a proprioceptive phenomenon. Quick stretches elicit the greatest reactions in these reflexes, as in the motor salivary response. In both cases the source of greatest stimulation is in the change from contraction to relaxation. Both reactions are characterized by recruitment. Under continuous stimulation from an afferent nerve the reaction progressively increases.

As additional evidence for our conclusions we call attention to another more definite source of information too frequently neglected in psychological studies. An examination of the neurological data concerned with the behavior under discussion shows that the connections of the various branches of the trigeminus nerve which supplies the salivary glands have been carefully studied and their relationship to the muscles of mastication noted. The most recent research in this field is an experimental study by Whitehead.[4] The literature reviewed in this report shows that investigators in recent years have found both ascending and descending fibers in the mesencephalic root. The ascending white objects. In Priest's method the stimulus was presented alone, many times, with slight color change. *O's* judgment was influenced by his recollection of the preceding image. In the present experiments one compared the color of the Purkinje with the primary image, the one following the other very rapidly provided the Hering had been suppressed. When the Hering was present it was very fleeting and received very little attention from *O.* Since the Purkinje by its very nature is complementary, there is (ideally) only one color of stimulus which it could match namely, white. The method therefore held the promise of a determination of an absolute rather than a relative white, independent of personal judgment of color and dependent only on *O's* ability to match two colors seen in rapid succession.

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[4] D. Whitehead, *Central Connections of the Lingual Nerve in the Cat*, Master's Thesis, Cornell University, 1928.

Apparatus. A full description of Judd's apparatus, with diagram, has already been published,[6] and will not be repeated except for the mention of certain modifications and additions. A more intense light source was found necessary in order to arouse a Purkinje image that showed decided color other than violet from a nearly white source, for even a white source if of low intensity will produce a violet or bluish after-image. For this reason a 1000-w. lamp was substituted, with a much larger area of spiral filaments. The vertical length of filament was approximately 1 cm., the width across all filaments 1.3 cm. In order to prevent as much as possible loss of light flux from the larger filaments through the ½ in. apertures of the diaphragms, two steps were necessary: removal of diaphragm E,[7] and a focussing of the filament image farther along, in the plane of G. In the rear of the lamp house was placed a concave mirror, of aperture 18 cm. and focal length 10 cm. such that the lamp filaments were at the center of curvature. This also served to increase the intensity of the beam.

The filters that were used to alter the color of the stimulus were placed in the plane marked by H. These filters were made by the Eastman Kodak Company and were loaned to us by Professor H. E. Howe, who had calibrated them at the Bureau of Standards in terms of color temperature from a standard lamp.

Only one diffusing glass (about 30% transmission) was used in position I, and one 5 cm. diaphragm in position K.

One important addition to the apparatus was found essential, due to the stray light in the room. A house for the observer was constructed with blackened walls and equipped with a ventilating fan. A head rest was also used in place of the eye guide. The little lamps that produced the field illumination were concealed back of the *O,* along the sides of the house, thus illuminating the field indirectly by diffuse reflection from the walls.

Originally it was planned to vary the color temperature of the lamp in rather small steps by changing its voltage, and to keep the brightness of the stimulus the same for all color temperatures by use of a neutral wedge. The latter proved to be undesirable, however, as the necessary reduction in the brightness of the stimulus produced a decrease in saturation of the Purkinje image, to which the *Os* greatly objected. Too great an intensity, on the other hand (this could be produced for yellow stimulus only), had to be avoided, for it served only to blind *O* and render him incapable of making any judgment satisfactory to himself regarding the after-images. With regard to the first point mentioned, eventually it was decided to use only five different color temperatures, which were measured in the following manner. Howe's lamp had been calibrated (color temperature against voltage) both with and without a certain 'daylite' filter; also its color temperature was known at a single voltage for a number of other filters. This standard lamp, with the 'daylite' filter, was color-matched on the photometer bench by means of a Lummer-Brodhun cube, with the 1000-w. lamp and one of the other filters. In this way the calibration curve of the latter combination could be determined. By using the various filters in turn with the two lamps, and extrapolating the curves, the color temperature of the 1000-w. lamp in combination with any filter could be found. The following table, Table I, gives the values so found, with estimated error due to the use of the extrapolated parts of curves. Column 2 gives the color temperature (degrees absolute) when the filter is used with the standard lamp running at 96.7 volts; column 3 gives the color temperature when the same filter is used with the 1000-w. lamp at 100 volts; and column 4 gives the total transmission of the filter.[8] Column 5 gives the brightness of the stimulus, the measurement of which will be explained in detail later. Column 6 gives, in Munsell notation, the color characteristics of the stimuli. This latter specification is to be taken only as roughly designating the color.

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[6] D. B. Judd, Apparatus for the control of stationary light stimuli, this JOURNAL, 38, 1927, 107-112.

[7] Cf. Judd's diagram, *op. cit.,* 108.

TABLE I

SHOWING THE VARIOUS CONDITIONS AND CHARACTERISTICS OF THE STIMULI

Filter Number	Color Temperature °K		Total Transmission of Filter	Brightness in Millilamberts	Color Munsell Notation	Color of Stimulus
	Standard Lamp	1000-watt Lamp				
No filter	2848°	2950 ± 10	1.000	250	YR 7/5	Yellow
1	4234	4190 ± 40	0.286	71	YR 7/2	Less Saturated Yellow
2. Daylite	5550	5300 ± 20	0.165	41	P 1/4	White
3	6100	5460 ± 20	0.184	46	P 1/4	White Unsaturated
4	6680	6040 ± 20	0.147	37	P 3/6	Blue
5	8450	7175 ± 20	0.106	26	—	Blue

Color temperature specifies the temperature of a 'Black Body' radiating energy with approximately the same spectral distribution as the source which it matches in color. Since this spectral energy distribution for any given temperature may readily be found,[9] an analysis of the source with regard to

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[8] These values were also determined by Professor H. E. Howe.

[9] This value may be computed from Planck's equation or it may more easily be obtained from the graphs of Planck's equation that have been plotted on a large scale by M. K. Frehafer and C. Snow (Tables and graphs for facilitating the computation of spectral energy distribution by Planck's formula, *Bureau of Standards Misc. Papers*, 1925, no. 56), and that are obtainable from the Supt. of Documents, Gov. Printing Office, Washington, D. C.

They were followed in the usual way by the third negative and third positive phases. After the primary image and succeeding dark interval, the bright center (brightest part of stimulus) was the first to appear. This became surrounded by dark and bright rings, which rapidly contracted upon the center, seeming to replace it.[11] New rings appeared to come in from the periphery. Some of the *Os* described the motion as due to new rings superseding the old ones. The center was the last part to disappear, partly due to the usual foveal effect of retarded reaction.[12] The Hering image was doubtless in that first flash of the center, for it was colored like the primary, but was so fleeting that it got hardly any attention at all, due to the distracting effect of the moving rings.

With a color temperature of 4100° K (yellow), most of the brown-haired observers obtained a yellow Hering (center) and separated from it by a dark ring, a ring of highly saturated cobalt blue. It seems very probable that this is the Purkinje image. The *Os* who had observed in Judd's experiment had no hesitancy in calling these images by name, without previous instruction. The rings are not easy to observe and describe, for they last only a little more than a second, and only those with very noticeable characteristics receive attention. The other rings are a set of unsaturated light and dark blue circles. In spite of the brightness of the center, it received less attention than the Purkinje phase; hence the *Os* had an excellent opportunity to compare its hue with that of the primary. The Purkinje image of the red-haired *Os* for this stimulus was frequently pronounced purple and lavender.

For color temperature 5300° K and 5460° K, the response as to color of stimulus was white, or cream color, or without hue, or very unsaturated. The corresponding Purkinje after-image was an unsaturated blue, usually desi[illegible]ted as bluish-white, the dark circles in this case appearing gray. Occasiona[illegible] response was yellow for the Purkinje. Some reported a yellow Hering and blue Purkinje after-image which, so far as one could judge, were complementary. For carefully chosen *Os* this may afford a criterion for a white stimulus, for ordinarily the Hering and Purkinje after-image are not complementary, any more so than are the primary image and the Purkinje. Only the very inexperienced *Os* saw, in their first trials, only white and gray after-images. Again the red-haired *Os* saw purple, pink, and lavender both in place of and in addition to the blue.

The observations do not agree, apparently, with those of Fröhlich, for whom a strong white stimulus yields a white Purkinje after-image. His stimulus may have been of different brightness, however, from those used in this experiment. It may be that a white Purkinje may still be available from a white stimulus of suitable intensity, but not with this moving ring response. Most *Os* objected to a brighter stimulus (for 2950° K—the only one that could be made more intense with this apparatus) as blinding them.

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[11] A. Samojloff mentions (Zur Kenntniss der nachlaufenden Bilder, *Zsch. f. Psychol.*, 20, 1898, 118-125) images of a similar nature from a peripheral stimulus.

[12] The after-image series described here are much like those that Fröhlich obtained from strong stimulation. Cf. Fröhlich, *op. cit.*, *ibid.*, 53, 1921, 79-107.

On reducing the intensity of the stimulus the details of the flash-images were blurred and confused—a blotchy gray blur of light resulted. This might possibly correspond to Fröhlich's white Purkinje.

For color temperature 6040° K, those to whom the stimulus was definitely blue reported a yellow after-image; those who called the stimulus bluish or greenish, reported only bluish after-images (very unsaturated). Perhaps a higher intensity would have evoked a yellow Purkinje for them. To these *Os* even a stimulus of much higher color temperature, 7175° K, failed to give the yellow image. One O reported the Hering and Purkinje after-images reversed in position, the latter yellow and appearing in the center, while the former, definitely blue, appeared near the edge.

One conclusion to be drawn from these blue stimuli is that, for unsaturated hues, blue is no more effective than any other color for evoking a complementary Purkinje. Previous work has shown that for saturated stimuli blue is apparently the most effective hue. The second conclusion is that this failure of more than half the *Os* to obtain the complementary image from the blue stimulus seems to preclude the possibility of using this method to determine a white stimulus that will evoke a Purkinje similar to itself. For unless this image goes through a white stage as the color temperature of the source is raised from 4100° K, the 'white' stimulus could not be defined, except by particular *Os*, to within 1000° K or more.

One other point worthy of mention with reference to the blue stimulus 6040° K was a garnet-colored ring sometimes appearing near the periphery of the flash-image, occasionally seen by the author. But the auburn-haired *Os* reported pink, pinkish lavender, purple, and violet in the rings, regardless of whether they had pronounced the stimulus yellow, white, or blue. The location of this 'red ring' region on the retina, if there is any such, may have some important bearing on the theory of this subject.

Other results worthy of mention are: (1) After-images were reported from small stimuli, covering 1.2° and 3° respectively of the retina. In the former, covering the fovea centralis, no complementary image was observed for these brightnesses. In the latter, both Hering and Purkinje after-images were observed. (2) A stimulus that showed two distinct hues yielded two different hues in the flash-image.

Summary. With graded stimuli less than 70 ml. in brightness, of short duration, and low saturation, the following results were obtained.

(1) White stimuli 5300-5460° K usually produce blue or purple Purkinje images, rarely white.

(2) Blue stimuli 6040° K usually produce blue Purkinje images, yellow to only a few.

(3) Yellow stimuli 4100° K seem invariably to produce blue Purkinje images, of rather high saturation.

(4) This image is not strictly complementary to the stimulus.

(5) Red-haired *Os* report red (purple, pink, lavender, etc.) in nearly all after-images; brown-haired *Os* rarely reported this hue.

(6) The Purkinje image fails to appear in the fovea centralis, but appears in the parafoveal region.

red, and contrariwise. It appears, then, that so far as our *Os* had any color categories they followed the 'primary' colors red, yellow, and blue, with orange, green, and purple sometimes used on a par with the 'primaries' and sometimes ignored altogether. This follows the usual casual and uncritical treatment of colors.

MAIN SERIES

Procedure I. Our problem, now, was to force our *Os* to develop, each for himself, sets of color categories which would be adequate for any problem of color comparison and discrimination that we might set for them. To do this we added to the presentation a second standard which defined further the limits of the series in which the variable moved. Ten series by the method of limits were run through for 16 or 18 combinations of the four hues with each other, with black, and with white.

TABLE II

SHOWING THE LIMINAL DETERMINATIONS IN PERCENTAGES FOR THE VARIOUS COLORS USED IN THE DIFFERENT COMBINATIONS

R = Hering red; Y = Hering yellow; G = Milton Bradley green; B = Hering blue; W = Baryta white; and Bk = Velvet black

O	R to Y (%Y)	Y to R(%R)	B to G(%G)	G to B(%B)
L	2.5 ± 0.5	4.8 ± 1.8	18.5 ± 1.2	19.3 ± 3.3
N	4.0 ± 1.2	57.3 ± 1.9	11.5 ± 3.5	9.5 ± 2.8
R			15.0 ± 1.5	14.8 ± 0.9
	G to Y(%Y)	Y to G(%G)	B to R(%R)	R to B(%B)
L			2.5 ± 1.4	2.1 ± 0.5
N	5.2 ± 2.0	5.0 ± 2.0	1.9 ± 0.7	2.8 ± 0.5
R	5.3 ± 0.5	1.0 ± 0.3	2.4 ± 0.6	1.1 ± 0.4
	x to Bk(%Bk)	Bk to x(%x)	x to W(%W)	W to x(%x)
L	6.7 ± 1.0(x=R)	2.5 ± 0.2(x=R)	1.0 ± 0.2(x=Y)	6.8 ± 1.4(x=Y)
N	13.5 ± 1.2(x=Y)	3.8 ± 0.6(x=Y)	5.0 ± 2.0(x=G)	1.5 ± 0.5(x=G)
R	41.8 ± 1.4(x=G)	1.5 ± 0.5(x=G)	1.5 ± 0.5(x=R)	22.5 ± 1.5(x=R)
	R to G(%G)	G to R(%R)	Y to B(%B)	B to Y(%Y)
L	4.8 ± 0.5	17.5 ± 0.2	5.5 ± 0.8	2.6 ± 0.2
N	3.8 ± 1.8	32.0 ± 3.0	4.5 ± 2.0	1.8 ± 0.3
R	15.0 ± 1.0	45.3 ± 2.3	18.5 ± 1.2	7.5 ± 0.5
	Bk to W(%W)	W to Bk(%B)		
L	1.0 ± 0.2	1.1 ± 0.3		
N	1.3 + 0.4	1.1 ± 0.2		
R	1.0 ± 0.5	3.8 ± 1.4		

Instructions. The *Os* were instructed: "You will be shown three colors—two standards and one variable—and you are to judge the relation of the variable to each of the standards. You will be asked to tell upon what you base your judgments."

Results. The rough liminal determinations are recorded in Table II. These values indicate that all *Os* are building up criteria for the comparison of colors, but that during this process there have been several shifts of attitude under the influence of prior concepts of color relations and of attitudes set up by the experimental conditions. For example, N did not report the presence of a resemblance to red in the yellow-red series until the color was well past the middle orange; L did not recognise the appearance of green in the blue-green series or blue in the green-blue series until a very marked change had taken place; R experienced the same difficulty with green and blue and failed, also, to note the change of white toward red and that of green toward black as readily as he did the others. Reasons for these judgments are apparent in the reports of the *Os* on their bases of judgment. N reported that "the (yellow) disk changed but did not begin to look like the other standard (red) until after it had passed through orange. I always think of orange as a color half way between red and yellow, and colors on the one side are red-orange and those on the other are yellow-orange." L was still of the opinion that "green is made up of blue and yellow" and though "the blue changed" he did not think it could be toward green. Similarly, R in many cases continued to name a color by its original and more insistent component, though "it looked quite different from the standard" until the added color became almost equally insistent. For him "white is still white even though it has a tinge of something else" and "a good green is a dark green."

The values in the cases of the complementary colors are especially important for our thesis. We find that in every case black and white seem to be treated as non-complementaries, *i.e.* white appears in the black, and black in the white with very slight changes from the standard. This might be significant negative evidence were it not for the fact that in 10 cases out of 12 the same results were obtained when the other complementaries were mixed; *i.e.* red became greenish; blue, yellowish; and contrariwise, with a slight decrease in chroma from the beginning standard. This indicates that the *Os* at this stage were judging the first change in the variable and assigning to this change the direction of the second standard without observing such a direction of change. The observations of the *Os* confirm this interpretation. All *Os* stated in similiar language that they "knew that the starting point of the series and the direction of change were given by the two standards" and that, consequently, they "reported the first noticed change in the variable" to be toward the second standard.

Our results thus far demonstrate how effective are our *Os'* ingrained notions about colors in determining their judgments of the presence or absence of color resemblances, and how prone *Os* are to adopt observational attitudes which are inadequate to the problem and which produce misleading results. These attitudes were evident early in the series, but we made no attempt to alter them by instruction. At the end of the series, then, we attempted to change the experimental conditions in such ways as to set up more critical and more adequate attitudes and criteria.

Procedure II. In this procedure we presented two standards with the variable as before but only one of them lay within the series of the variable.

Instructions. The *Os* were instructed; "You will be shown three colors, two of which are standards. The third will be varied serially. You are to judge whether the variable resembles either standard. When it is different increase the size of the limens and of the MVs. In the earlier series L avoided this effect by the use of secondary criteria which did not fit the problem. In the fourth series he became aware of this inadequacy, and began to shift his attitude so that he made 6 judgments under the first and 4 under the second attitude. This shift is evident in the size of the mean variation and is noted in his reports. From this point on L became more critical of his judgments and distinguished the gray portion of the complementary series as a distinct color which shows no resemblance to any of the six principal colors. The moment of the change of attitude in the other two *Os* is not as obvious, but both found it necessary to distinguish gray from black and white in the same manner as from blue and yellow. The case for gray, then, is just the opposite with our *Os* to that for orange, purple, and the other intermediates. The latter were used at first but soon dropped out, whereas the former was found necessary to an adequate description of the black-gray and gray-white series. This further substantiates our earlier findings and those of Rich, and justifies our treatment of gray as one of the fundamental colors.

SUMMARY

At the beginning of this experiment we found that our naïve *Os* had only incomplete and inadequate categories for the accurate description of colors. Following the errors of practical tradition, they assumed that green is blue-yellow, that gray is black-white, and that orange, purple, and violet are separate color categories. In the course of a long series of judgments of color-likenesses, all *Os* found that the necessary and sufficient color categories or unique colors are red, yellow, green, blue, black, white, and gray. This gives us further justification for treating gray as an unique color. The precise limits of this gray need to be determined by some such method as that suggested by Rich.

THE ILLUSION OF SIZE IN PIN-HOLE VISION

By Norman R. F. Maier, Long Island University

The apparent size of an object considerably decreases when the object is observed through a pin-hole, and the apparent distance between the object and the observer becomes correspondingly greater. The purpose of this investigation was to determine the factors involved in the illusion.[1]

Part I

First the effect upon the illusion of the following conditions was studied; brightness of object, illumination of the room, knowledge of the room, size of the pin-hole, absolute size of the object, and dispersion of light.

Apparatus and procedure. Two round windows in a light-proof box served as the objects of observation. They were covered with milk-glass screens and illuminated from behind. The light box was so divided into two compartments that one window appeared in each. The brightness of each of the object-lights could thus be independently varied, either by adding or subtracting ground-glass screens or by moving the lights within the compartments toward or away from the screens. The size of the object-lights could also be varied at will. The quality of the light was kept virtually constant. The window at the right (called the *primary* light) could be varied in size by rotating near its circumference a disk containing apertures of various sizes, 40, 28, and 20 mm. in diam.

The window to the left (the *comparison* light) had a variable camera diaphragm, with apertures variable between the limits 10 and 40 mm.

The light-box was placed at the end of a series of tables arranged along the center of the room. At the opposite end of this series of tables and 7 m. from the front of the light-box, a large black screen was placed. The screen had two windows 10 cm. square placed side by side and about 20 cm. apart. The window to the right was covered with a large, heavy cardboard disk which could be turned on its axis. Near the circumference of the disk were several small metal plates with pin-holes of various sizes drilled through them. The pin-holes were .03, .04, .05, .07, and .09 in. in diam., as determined by the size of the drill. By rotating this disk pin-holes of various sizes could be presented before the window where a head-rest was placed.

Along the centers of the tables a partition was set up so that the *comparison* light and the large window in the screen were on one side, and the *primary* light and the pin-hole on the other.

The *O* sat behind the large screen. Through the pin-hole at the right the *primary* light could be observed and through the large window at the left the

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[1] From the Psychological Laboratory of the University of Michigan. The experiments were done during 1925-26 under the direction of Dr. John F. Shepard.

comparison light. In order to make a comparison between the two object-lights the *O* had merely to move his head about 10 inches from one side to the other. The primary light could easily be localized through the pin-hole by the use of the head-rest, which also served to keep constant the distance between the eye and the pin-hole. The judgment was always made with the primary light as the standard. The comparison light was judged equal to it or larger or smaller. *E* varied the comparison light until a perfect match was found. *O* was permitted to make as many observations as he wished before reporting. Both increasing and decreasing variations of the comparison light were made in all cases.

To test *O*'s ability to make these comparisons, readings were taken with the disk containing the pin-holes removed.

The 18 *O*s in this and later parts of the investigation were students, assistants, and instructors in psychology and an instructor in physics.

Results. (1) Individuals differed in the matches made between the primary and comparison lights. The range was 17 to 34 mm. of the comparison light to match a 40 mm. primary light observed through the pin-hole. When the pin-hole was not used, the range was 39 to 41 mm. of the comparison light to match the 40 mm. primary.

(2) For *O*s who had never before been in the room there was a tendency slightly to exaggerate the illusion, *i. e.* the matching comparison light was physically smaller before seeing the room than after seeing it. This increased reduction was as high as 4 m.. for some *O*s, for others only 1 mm. with the 40 mm. primary light.

(3) As the area observed was made smaller, the percentage of reduction by the pin-hole slightly decreased. The diameters of the primary lights used were 40, 28, and 20 mm.

(4) There was a general tendency for a slightly greater reduction for smaller pin-holes, *i.e.* a greater illusion. Five pin-holes were used.

(5) When the primary light was 100 times the brightness of the comparison light it was still seen as smaller. Differences in brightness tended to make the brighter look larger within a range of 2 mm. Beyond this an increase of 6000 times showed little or no difference. The reduction of intensity caused by the pin-hole was never 100 times, so the light observed through the pin-hole was brighter for all readings. When *O* equated both brightness and size, the actual physical difference in brightness was never more than 7.5 to 1.

(6) When both brightness and size were matched by *O* an increase in the intensity of the light observed normally made it appear somewhat larger, a decrease smaller. Within small limits size and brightness were often confused.

(7) In order to test the spread of light beyond the edges of the area, a factor possibly present under normal observation and absent in the pin-hole, two points of light (40 mm. from center to center) instead of an area, were used. Were the spread of the light a factor, it would make the points observed normally appear closer together (or at least not farther), and thus the effect would be opposite to that present in the case of the area. It was found that the pin-hole caused the points to appear closer together to the same extent that it caused the area to appear smaller.

PART II

When both observation lights were reduced in intensity in like proportion, there was less illumination in the room and it was found that the reduction in size caused by observing through the pin-hole had become slightly less. It was then thought that the environment might be a factor, since the brighter light tended slightly to illuminate the room. When observing through the pin-hole the environment was completely removed, whereas under normal observation the slightest amount of illumination might serve as a factor. The arrangement was, therefore, changed for a better control of the surroundings.

Procedure. Two rooms were used. The primary light remained in the room already described and the comparison light was placed in a room to the left. The rooms were joined by large doors so that the second light could be seen by merely turning the head 60° to the left. Thus the rooms formed a sort of 'L,' *O* sitting slightly behind the junction joint. *O* was surrounded by black cloth screens about 1 m. distant so that he was unable to determine the nature of the rooms. The object-lights were seen through windows in the black cloth background so that there were no environmental cues.

The environment for the comparison light was controlled by (1) having the room absolutely dark, (2) stringing from the point of observation to the object-light 8 small lights along the line of vision, lights which could be brightly lighted or dimmed to red spots, and (3) placing a screen in the plane of the comparison light, the screen illuminated from behind as desired. Thus it was possible to remove the environment, to bring the environment between the *O* and the object, or to have the environment on the same plane as the object.

Results. With an environment present for the comparison light, the reduction caused by the pin-hole was slightly greater, and it was greatest when the environment was between the *O* and the object, *i.e.* to make a match it was necessary to have the comparison light smaller with an environment than without. The comparison light always seemed farther away when the environment was removed and yet it required less reduction to match the primary light. Changes in size were also seen as changes in distance. Thus increasing the size of the comparison light made it come closer. The environmental factor was less important in the perception of size than in the perception of distance. It was evidently a minor factor in the pin-hole effect as the greater part of the illusion remained with the environment entirely removed.

PART III

The question was raised as to whether knowledge of the fact of looking through a pin-hole in any way affected the judgment of size.

Procedure. To test this query, *Os* ignorant of the conditions were used. *O* entered the room in the dark, was seated and told to place his head against the head-rest, slightly moving his head against it until a light appeared. The room was absolutely dark and the *O* believed that when his head was placed correctly it caused a contact which brought on the light.

Results. Two *Os* made observations under these conditions and later under normal conditions. The change in technique made no difference in the readings.

Part IV

One can voluntarily change the size of the object observed through the pin-hole by changing one's accommodation. It is possible to get a clear image of an object through a lens, though the lens be out of focus, with the aid of a small aperture. Thus the pin-hole serves to give a clear image no matter what the accommodation is, and the retinal image is made smaller when the lens of the eye is accommodated for some point nearer than the object observed.

To test the importance of this factor a pin-hole was placed near the edge of a piece of tin. *O* was then asked to report concerning the object-light seen through this pin-hole while looking at a light-cross past the edge of the tin. The cross was placed at various distances and *O* was asked to look at it and at the same time to estimate the size of the object-light seen through the pin-hole in relation to that seen normally. The observations were somewhat uncertain. The object-light was still somewhat reduced even when the cross was at the same distance.

Parallel lines were then observed under the same conditions, one end observed through the pin-hole the other end past the edge of the tin. The lines could then be seen to be the same distance apart, both through and past the edge of the tin. That is, the parallel lines looked to be continuous. If the eye was gradually so moved as to see the lines past and then through the pin-hole, the lines were also continuous. On the other hand, when the eye was deliberately accommodated for a near point, so that the ends of the lines seen past the edge became blurred, those seen through the pin-hole were still clear and seemed to move nearer together than the blurred ends.

It is a well-known fact that, in the case of monocular vision, accommodation for a nearer point causes a given object to appear smaller and more distant.[1] But we see in this case of the parallel lines an additional and more extensive reduction in size due to vision through the pin-hole.

By observing through a slit, objects are seen as narrow and tall with the slit vertical, and as wide and short with the slit horizontal. When a round lighted area is observed the same is true; but it is now seen that the sides are blurred in the direction of the slit. This shows that the accommodation is not for the object observed but for some point nearer. The narrow dimension of the slit gives a clear image in spite of the lack of accommodation; whereas the large dimension lengthwise of the slit gives a blur. This blurring was less noticeable when objects rather than lights were observed.

It was also found that as *O* moves closer to the object there is a point of no-reduction in size of the object-light when observed through a pin-hole. This distance is generally about 2 m. from the object.

Helmholtz pointed out that an object observed through a pin-hole is seen as larger if the object is very close.[2] In such a case the accommodation is for objects farther removed than the one observed. The pin-hole prevents blurring and the object is seen as larger because of the large retinal image. *O*s under atropine (and thus without accommodation) were also tested. A far-sighted *O* had little or no reduction in the size of the object-light observed through the pin-hole. Others with astigmatism and myopia could not get clear enough images with normal observation to make comparisons with the object seen through the pin-hole. As we were limited in *O*s, we were unable at the time to make a complete analysis.

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[1] See H. Helmholtz, *Physiological Optics*, Eng. tr., III, 389.

[2] *Op. cit.*, I, 127.

In summary, the facts presented seem to show that, under our conditions, decrease in brightness and elmination of environment through the pin-hole are small factors in the result; but that the 'illusion' is largely due to a reduced retinal image on account of combination of near accommodation with the pin-hole between the object and the dioptric system.

APPARATUS

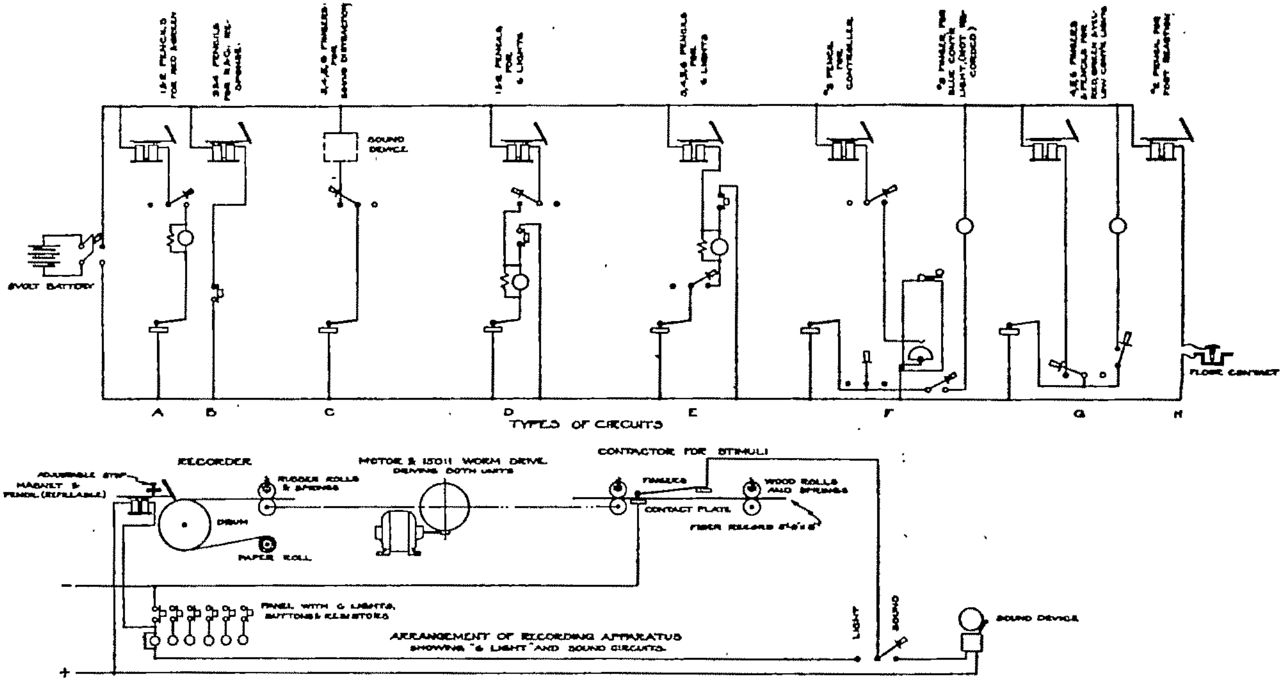
A READILY ADAPTABLE APPARATUS FOR GIVING AND RECORDING STIMULI AND RESPONSES

By E. E. BRAKEMAN and C. S. SLOCOMBE, Personnel Research Federation

In performing certain experiments, it was necessary to obtain apparatus that would record both onset of stimulus and response on the same paper, and that would also record time to 1/25 sec. without the use of a chronoscope. Recent experimental work was reviewed in the attempt to find such apparatus, but it seemed that there was none that overcame all difficulties. Consequently the apparatus described in this paper was designed and has so far proven quite satisfactory. This apparatus is comprised of three parts, *(1)* a small electric motor (see Fig. 1), *(2)* a recording drum, and *(3)* a device for automatically setting up stimuli.

(1) The electric motor (M) is shunt-wound, 110 v., and 1800 r.p.m. It is geared down by means of a worm-driven gear (G) to 3 revolutions per sec. In this set-up there is no clutch between the gear and the rollers which it drives, but it would be advisable to have one. The ratio of the reduction of the speed of the motor is determined by the rate at which the paper must be run through. When this has been decided upon, the size of the driving roller and the ratio of the gear are arranged accordingly. Paper in this set up is run through at the rate of 1 in. per sec., the circumference of the driving roll being 3 in. and making 3 revolutions per sec. With this speed 1/25 in. may be accurately measured. If it were desirable, however, to measure shorter time intervals of, say, fiftieths or hundredths, the paper could be run through at a faster rate. A shaft (T) from this motor is attached to two rollers (R) and (O), roller (R) running the recording apparatus and (O) the apparatus for giving stimuli. The speed of the motor is adjusted to exactly 1 in. per sec. by a resistance in circuit with it.

(2) To record responses a roll of paper (P) is mounted on a wooden roller with round grooves in each end into which fit metal points. The degree to which these metal points press against the wood and allow play in the roll is regulated by a screw on one end. The paper (P) is run off around a metal drum (D) and back between rubber rollers (R) mounted on the same wooden support as (D) and (P). Of this pair of rollers one is run by the motor, the other is an idler. The degree to which they press against each other is determined by the thickness of the paper and is regulated by a spring and a screw (S) from the top which presses them together. Just above the drum (D) are copper bronze fingers (F) 5 in. long, on the ends of which are soldered small adjustable Eversharp pencils. These fingers are pulled down by small electromagnets (E) when a current passes. To prevent oscillation in the fingers when they are pulled down and released, a screw (Y), driven through a strip of wood just over the finger, dampens and regulates the freedom of the fingers.



NOTES.

SIMPLE REACTION—CIRCUIT "A" IS USED FOR STIMULUS, "B" FOR RESPONSE, & "C" FOR 4 SOUND DISTRACTIONS.
CHOICE REACTION—CIRCUIT "A" IS USED FOR TWO COLORS, "B" FOR TWO RESPONSES.
ATTENTION—CIRCUITS "D" & "E" ARE USED FOR 6 LIGHTS.
CHOICE REACTION ON CONTROLLER—CIRCUIT "F" IS USED FOR CONTROLLER AND UNRECORDED BLUE LIGHT, AND "G" FOR THE OTHER RECORDED STIMULI, GREEN, YELLOW & RED.
FOOT REACTION—CIRCUIT "H" IS USED FOR RESPONSE RECORD AND "C" FOR STIMULUS & 6 SOUND DISTRACTIONS.

FIG. 1. DIAGRAM OF STIMULATING AND RECORDING APPARATUS

(*S*). To give the stimuli, a heavy compressed fiber is run through the two free rollers (O′), passes over the metal plate (A) and under the metal contact finger (X), and runs out through the rollers (O). The pressure and play of these rollers is regulated in the same manner as in rollers (R). The metal contact finger (X), is a flat piece of copper bronze with a small bronze roller ¼ in. in diam. on the tip. In the fiber record slits 5/16 in. long are cut at such intervals as the stimuli are required.

Diagrams of circuits show those for giving and recording stimuli and others for recording responses. Thus when a slit in the fiber record passes over the metal plate (A), the roller drops into it and making contact, closes the circuit (A), for instance, and gives the stimuli. The current passes through (E), pulls down (F), and a dot is made on the record. When *S* responds, the circuit (B) is closed, current passes through (E), (F) is again pulled down and a line is made on the paper. Circuit (B) is 4 volts, that being sufficient to pull down the pencils (F); circuit (A) is 6 volts in order properly to heat the filament in the lamps. They are each run from a storage battery.

For recording, squared paper of 1/25 in. squares and 7 in. in width is used. The distance between the stimulus dot and the beginning of the response line may thus be accurately read in 1/25 in. *i.e.* 1/25 sec., without the use of a chronoscope. The fiber for giving the stimulus is cut 7 in. wide, ruled vertically in half inches and horizontally at intervals at which the slit is cut and stimulus given.

Six pencils and 6 contact fingers were built on this apparatus, enabling 6 sets of stimuli and 6 sets of responses to be given. It is flexibly wired so that any combination of stimuli and responses may be recorded. If for instance when only one light is flashed and the response comes very quickly after the stimulus, onset of stimulus may be recorded on one pencil and that of response on another. In experiments where 6 contact fingers are not used for giving stimuli, the free ones may be used to give distractions.

The apparatus is used at the present time for giving the stimuli and recording the responses in the following experiments. (1) Simple reaction to light stimulus with hand response. (2) Simple reaction to light stimulus with foot response. (3) Choice reaction, simple and reversed. (4) Specific fatigue, and oscillation of attention, by requiring the *S* to respond by appropriate key depression to the lighting of any one of 6 lights in random order and quick succession. (5) Muscular adaptation, by requiring the *S* to move a handle (a street-car power controller) through 240° over 8 notches at a stated speed, with varying resistance to the movement, controlled by a brake-band. (6) Stereoscopic vision, by a modification of Spearman's bioscopic apparatus. Two dark gray glass plates are set up before an evenly illuminated screen; the slow but variable movement of one is controlled by a cam; the *S* is required to move the other to keep it in the same plane. Both (1) and (2) are given with and without distraction by 4 kinds of noises.

These are the uses to which this apparatus has been devoted, but it can be readily seen that any combination of stimulus and response may be used. Either lights or sounds may be used for stimuli or distractions, with either a foot or a finger response. Moreover, it answers the major requirement of automatically giving and recording stimuli and responses in such manner that the results are readily available for later analysis. It is very convenient and easy to handle, since there is no inking device. The records may be readily revised for giving stimuli in a different way.

A DEMONSTRATIONAL MASSON DISK

By LEONARD CARMICHAEL, Brown University

The demonstration of the fluctuating bands of the rotating Masson disk is difficult in a large lecture hall if the disk is constructed as it is ordinarily figured in the textbooks of experimental psychology. The difficulty arises because the inner darkest band is still not distinct enough for the persons at the back of the room, and the outer lightest band is not faint enough for those in the front of the room. The demonstration can, however, be made if the disk be larger and the black segments that create the bands, instead of being made all of the same size, diminish in size from the center out, so that the range of difference is greater.

The writer has found the following satisfactory at distances from 6 to 40 ft. The radius of the disk is 20 cm. About a radius as an axis of symmetry a quadrilateral is constructed, tapering toward the periphery of the disk. The base of this quadrilateral is 4 cm. along the diameter perpendicular at the center to the radial axis; the width of the quadrilateral at the extreme periphery of the disk is 2 mm. Within this area nine annular segments are laid out. Each segment is one cm. wide. The segments are separated from each other by spaces of one cm. Each is terminated at the bounding lines of the tapering quadrilateral. The segments on the white cardboard are filled in with black India ink.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Psychology: Its Facts and Principles. By H. L. HOLLINGWORTH. New York, D. Appleton & Co., 1928. Pp. xviii, 539.

This book is evidently intended as a rather serious attempt to re-interpret the main data and principles of psychology and to harmonize the various points of view. In a preface for psychologists, the author explains that the view is not "structuralism," because it deals with activities and processes; that it is not "mere behaviorism," since it does not limit itself "to the account of visible changes" and "insists that aches and pains receive the consideration to which their occurrence entitles them;" and that it is not "associationism," despite certain superficial resemblances, for "it is not 'mere ideas' that are associated but primal events in nature." The author, moreover, holds that he has covered the facts recently emphasized by the *Gestalt* psychology, without special pleading, and has escaped the several vices of the philosophy of mind developed by this school. The book omits any treatment of neuro-physiology. The author regards such data as belonging to another field whose facts have not yet been worked out sufficiently well to show significant correlations with psychological facts. He finds in the psychological facts themselves a unifying principle which everywhere appears, "in whatever psychological activity one begins description." This turns out to be the old principle of redintegration about which Sir William Hamilton, nearly a century ago, was as enthusiastic as the author now is, and which was so fully elaborated by James Mill as the law of association. Here, of course, it is given a modern setting. This principle of redintegrative sequence, we are told, is "at once the criterion, the mechanism, and the dynamics of mind." Once it is accepted, there is left only the matter of referring all sequences to it. This satisfaction with a simple principle, itself not well accounted for, seems to the reviewer to be the weakest point of the book. To this we shall come back later.

The author's psychology is frankly descriptive, "and therefore in the only scientific sense explanatory." This 'therefore' phrase seems to be another wholesale method of settling a point that may well be questioned. Indeed the reader will doubtless be impressed by the author's general tendency in later chapters to fall into a superficial description which really is to a large extent inference from the principle rather than a detailed picture of what really goes on in any case. We shall see. But first, more as to the general point of view. The author first inquires into the question as to what mental processes really are. What are subjective events? He rightly deplores the loose use of the term 'mental,' and limits this term so as to make it practically synonymous with 'redintegrative sequence.' "By a mental process or a redintegrative sequence, then, we shall mean a series of changes in which a partial antecedent functions appropriately or adequately for a former antecedent of greater complexity" (p. 6). Physical sciences, we are told, are primarily concerned with non-redintegrative sequences, while psychology is interested in sequences which exhibit the redintegrative pattern and also in natural events participating in these sequences. The distinction is admitted to be only a general one, and the various biological sciences come between these extremes to make the transition gradual. The distinction, then, which the author illustrates and elaborates, seems to amount to this: the occurrence of any given 'physical' event, such as the momentary electric oscillations between the pole of a Leyden jar and a metal pencil held near it, is less likely to be accompanied and followed by the group of events in which a similar occurrence of oscillations in a previous 'flash' of lightening had its setting and by which it was followed, than is the presentation of a symbol on a card (before an observer) to be accompanied by the salivation, orientation tendencies, images, memories, pleasures, etc., by which certain features of this symbol had been previously accompanied and followed. Moreover, when the symbol is presented before different individuals, each one reports distinctly different redintegrative patterns. Thus mental events are more variable, too, than physical events. The grouping together of such 'events' as salivation and orientation tendencies with images, memories, and pleasures, does not bother the author at all, for all of these are alike events in the general "continuum of nature." Whether they are conscious events, as pains, sensations, etc., or physical events, makes no real difference, since all alike tend to become aspects of the "public world." 'Subjective' and 'objective' are categories, after all, which refer merely to differences in the consistency or privacy of the events. There is, however, no necessary relation between mental processes and subjective events. These are merely two relatively independent classifications.

A good deal may be said for these relative distinctions, based on redintegrative sequence, consistency or privacy of events, etc., but when the author regards ' a fear,' 'a memory,' 'a sensation,' etc., as in a way coördinate with a flash of lightening, or a rain, he seems to the reviewer to be liable to the use of bad methods as a consequence. It is easy enough to say that all science is 'descriptive,' and that all explanation in a real sense rests on a 'frankly descriptive basis.' But what *is* description? Is it merely sequential enumeration? Or must it, *to be scientific*, not take note of whole circumstances which are not so immediately observable to anyone, such as the differences of potential in connection with the electrical phenomena, the numerous bodily adjustments *of an individual* to certain external circumstances in the case of the fear, the memory, etc. Surely we cannot overlook these conditions and processes as part of the thing to be described. Neither the 'mental' nor the 'physical' events are mere floating incidents. The former are always somehow related to the stimulation and the response adjustments of an organism and are only aspects of the adjustments, as are also nerve structures and currents, inhibitions, facilitations, etc., while the latter are likewise not single, unattached events. The 'behavior of a fear' can certainly not be treated scientifically without consideration of stimulating circumstances, frustrations of physiological processes, sets, etc., in a complex organism which has been a somewhat unitary event in the continuum of nature for many generations. Science has made its best advances in man's endeavor to *predict* and *control* these processes as the result of experimentation. The book before us suffers not a little from a neglect of this fact. It consequently reads more like an introduction to philosophy than a text-book in science, and the few generalized platitudes of descriptive formulations do not seem to function well when we come to account for such 'events' as the *elimination* of certain response tendencies in learning to the advantage of others. Such topics, and experimental techniques devised to test out various hypotheses generally, are too much neglected. The student using the book is in danger of becoming a philosophizer instead of an experimenter. In justification of this criticism it may be pointed out that many of the experiments which are described and discussed are merely illustrative data and are therefore more or less selected for the convenience of argument. This is, however, to be expected in a book with the purpose of the one before us.

Space does not permit us here to enter the minutiae of redintegrative laws. Any particular experience is always complex; it can never actually recur. What does recur is only "another event in a given class" or series in which the former experience also had membership, as the "same" day of the week. Actually, of course, the day itself is not even the same; but there are elements in common—partial identities— by which the new revives the older event. The response, then, "follows a detail of its former antecedent," the completeness of this revived response depending upon the completeness of the stimulus. But there are various qualifying conditions, such as the prepotency of certain elements of the original experience, the reënforcing or inhibiting effects of different factors operating together or in opposition, the greater readiness of some feature than of others for redintegration, etc. Thus there are many special conditions to consider. These matters, with some applications to special phenomena like synesthesis and to language, take us over nearly one hundred pages of the book. The account of the nature, genesis, and function of language is disappointing, due to the somewhat passive nature of the individual as conceived by the author—the too great emphasis on external stimuli as contrasted with stimuli due to metabolic changes in the organism. If the author were more open to "outside" aid he might have improved his treatment greatly by borrowing something of Jenning's 'spontaneity' in behavior. But he does show well in places how language symbols serve as controls of the behavior of others.

A chapter is given to natural signs and the perception of identity. After considering different views as to what is real with regard to appearances, colors, temporal relations, etc., the author concludes that "natural events exist wherever they are found, by whomever they are observed, and with whatever statistical consistency they are reported, namely, in the realm of nature. The *seen* pencil is the *real* pencil, although the visual features do not by any means exhaust it" (p. 115). The treatment of perception of identity is clear and logical and includes various experimental data, but it seems to the reviewer to suffer from the same over-passivity of the individual reactor (in as much as he enters at all) that was mentioned in connection with the treatment of language. The experiments, moreover, are used merely to illustrate the theoretical contention. "Thought and Things: The Nature of Ideas" is the heading of a long chapter in which the ideas or thoughts are treated as occurrences in the same realm as the things for which they stand. The erroneous notion of an 'inner world' as different from the 'outer world' is attributed to the fact (1) that adults can think of things in the world, however remote, without maps and other objects to represent these things, and (2) that the materials employed in this inner world seem to be usable by only one person. This duality, however, clears up with an analysis of the origin of these two conditions. Events are ideas only when they function as dynamic cues or antecedents and their power is attributable to their natural history, their association with the power of things for which they stand.

Two chapters are given to spatial and social perception. In these occurs a good deal of material derived from experiment, including the interpretation of facial expressions in photographs. Much space in later chapters is devoted to learning and retention (or memory), but the reviewer feels that the account of learning is in some respects superficial and that it avoids some of the most important problems in this field (indeed in the whole field of psychology), probably because they cannot be made to fit into the simple redintegrative formula. How will the author deal with learning which is contrary to both frequency and recency expectations, with intensity controlled? How will the redintegrative formula serve to explain the tensions and the peculiar narrowing of the subject's range of orientation in certain cases of confusion, and how will it function in processes of reconstructive thinking? A (mostly theoretical) chapter on imagination, dreams, and invention leads up to motivation, which is treated, in accordance with the author's general formula, as synonymous with the "antecedent, stimulus, or cue which initiates the process." Inner 'drives'—such as hunger pangs—are given as the basis of certain persisting motivations, but there is not sufficient recognition of the fact that several of these drives may be wholly outside of the individual's awareness. 'Motives' are persistent stimuli which when organized into a plan by means of symbols become 'purposes.'

A peculiar result of the author's point of view brings such topics as reflexes, habits and instincts under the head "Consequents of Mental Activity." Only small space is given to instincts, but the treatment is good and profits by the recent discussions of this topic. The great adaptability of the innate mechanisms in the human organism is emphasized. Despite the attention paid to motives, drives, cravings, etc., in connection with motivation, a full chapter of considerable length is later given to cravings, bodily feelings, and emotions, treated here, however, from a larger—though still rather introspective—point of view, with no inclusion of experimental data. In order not to identify Watson's 'rage,' 'fear,' and 'love,' as observed in infants, with the more precisely differentiated emotions of adult life, the author proposes for them the terms 'resistance,' 'startle,' and 'content,' adding a fourth (supposedly necessary) emotion—'gloom.' Later chapters cover the topics of reasoning, attention, voluntary action and decision, the senses, and intelligence. In view of our limited space the contributions in these chapters do not justify an analysis here. Appendix I is devoted to lists of classifications of reflexes, instincts, emotions, etc. by different psychologists; Appendix II, to rather extensive projects, questions and exercises for the several chapters, which are sure to be useful to instructors; and Appendix III contains references for supplementary readings, also arranged by chapters.

The general impression left on the reviewer's mind is that this volume is too theoretical and mechanical, and does not contain enough data (though it is rather too large) for a good general introduction to psychology. Its point of view is not that which emphasizes enough the experimental possibilities and methods of psychology; it leaves the student devoid of empirical means of working out his own problems. It will certainly not interest the great majority of college students and lead them into psychology as a career. For an advanced, critical course it will be more suitable as a general guide, though most instructors will desire to supplement it by the use of other works which this volume represents too superficially. As to its contributions to psychology, the reviewer cannot feel that it has solved the vexed problems of duality of 'mind' and 'matter' or made any one real contribution either in method or in content. The point of view is, however, clearly presented; definitions are well formulated; and many useful suggestions as to treatment of particular problems and data are afforded. It is impossible in a review to do entire justice to the author's complete presentation of the facts and principles of psychology, and to evaluate the possible effects of this book on the fallacious dualism of more carelessly written texts. The book merits wide and careful consideration.

J. P.

A State Educational System at Work. By M. V. O'Shea. Madison, Wis., The Bernard B. Jones Fund, 1927. Pp. xvi, 368.

This volume is a supplement to the author's earlier *Public Education in Mississippi*, and deals especially with the intellectual status and the educational progress of pupils in the elementary and high schools and freshmen in the colleges, both public and private, of Mississippi. It is a part of the reports on the educational survey of the State of Mississippi directed by the author, and carries, besides numerous tables and illustrative figures on the data obtained in the measurement work and in classifications, recommendations as to desirable modifications of educational procedure in the state. The book is very simply written and copiously illustrated so as to reach the general reader in the state. In this respect it appears to be a successful piece of work. The book might be read with profit by citizens of other states who should have a better insight into the procedure of surveys of educational systems, particularly as to the testing technique when carried out on a large scale. The psychologist and educator can in a short time get a fair idea of its contents from the tables alone.

The book as a popularizer of psychological and educational work is unfortunately not without faults, some of them serious. For example, a wrong conception of the "probable error" is given. This measure is defined and illustrated as follows: "One measure which indicates the limits of possible modification of our conclusion is known as the 'probable error,' or P.E. To illustrate: The median score of three thousand sixth grade white pupils in a Southern state is 99, say. We compute the P.E. and find it to be 0.8. This means that if every sixth grade white child in the state would be tested, the median score would not be changed from 99 by more than 0.8 either way—it would not be more than 99.8 nor less than 98.2, with the probability that it would fall at

99" (48 f.). This is, of course, entirely wrong. Passing over several conclusions relative to education, which we should be inclined to question, we may note that the results of the extensive mental testing with various group tests are handled most uncritically. Scores from various standard group tests at different grade levels are converted into IQs and handled as if all these IQs were alike and genuine. No attempt is made to determine the amount of discrepancies in results from different tests, or the effects of the lack of opportunities as compared with children from whom the norms are taken. The reviewer suspects, from his own experience with tests in the South, that chronological ages were very inadequately obtained, especially in the case of negro children. It is well known that the IQs from different group tests cannot be directly compared without considerable error, and also that some of them reflect much more than do others the effects of educational opportunities.

The survey finds the negro children much retarded in their school progress as compared with the whites, and a much more rapid elimination of negroes from school with advance of grades is also shown. "The difference between white and negro children in mental capacity is a real difference and is large." The difference varies throughout the twelve grades on large numbers from, roughly, 24 to 55 times its P.E. (p. 134). This is in conformity with what other investigators have found in the South, except that the difference here is probably larger than usually found. The size of the difference is, unfortunately, not easy to obtain, except in the IQs. On a total of 23,555 white children distributed through grades 1 to 12, a median IQ of 95.3±0.09 was obtained, while on a total of 4948 negro children similarly distributed the median IQ was 75.0±0.17, as the reviewer has figured these constants from data in tables on page 118 for whites and on page 132 for negroes. On the basis of this difference the author recommends a different educational regimen for the two races in the elementary and secondary schools, and also in the colleges (p. 5). This is probably a little premature in view of the many yet unsolved problems before the psychologist in race testing. A differentiated program for both races so that each child, irrespective of race, could advance regularly in the sort of work for which he is most fit, would doubtless be a much better recommendation. It would also meet the demands of any established race differences as these differences become determined. In view of the greatly inferior opportunities of the negro children in the Southern states generally, this recommendation by the author (based too much on mere scores without due consideration of relative opportunities) will doubtless have an unfortunate effect on the negroes, so far as future opportunities both in Mississippi and in other states are concerned.

The reviewer believes that the author's stress on advancement by mental rather than by chronological age is unfortunate, as it tends to justify the absurd practice of giving all children in each race the same training regardless of their several aptitudes and general abilities. If in the present survey attention had been given to more critical evaluations of the factors making for efficiency in mental tests—consideration of speed factors and sets for speed, of training in motivation, etc.—the author would doubtless have found a better explanation than he gives of the fact that the Mississippi children make a relatively good showing in the so-called achievement quotients, and that pupils in small towns with inferior educational opportunities especially stand high in this regard. An under-rating of intelligence scores, of course, will tend to bring about just such results. The conclusion that "pupils in rural districts may be less distracted from school work than are pupils in large communities, and that they may possess greater interest in school work and greater endurance, industry, and ambition to succeed, since they have rather meager educational opportunities as compared with pupils in larger places" would doubtless have been different if more thorough investigations even on relatively few cases had been made, and if the author had been better acquainted with the methods and the needed precautions in mental testing.

The usual sex differences were found: boys were a few months older than girls in all the grades of the elementary and the high school, except the first grade and the last two years of the high school, the difference in most cases being statistically reliable. The IQ of the girls was found to be higher than that of the boys, the difference being more than four times its probable error in most of the grades. The difference is reversed (and favors the men) in colleges but it is not reliable, statistically. The author believes that when training and ages are equalized, however, the sex differences between men and women will not be reliable. The tests used in the colleges—American Council Psychological Examination—undoubtedly show environmental and training effects. The differences found in the grades are in all probability due to the greater maturity in development of girls than of boys. The intelligence tests used in the grades and high schools in comparing races and sexes were Pintner-Cunningham Primary Mental Test for grades 1 and 2; National Intelligence Tests, Scale A, Form 1, for grades 3 to 8, inclusive; and Terman Group Tests of Mental Ability for the high school students. Certainly no serious student of race differences would be tempted to put much reliance on any of these tests as indicators of native differences, under the usual conditions of different training and cultural backgrounds.

J. P.

The Solving of Problem-Situations by Pre-School Children: An Analysis. By AUGUSTA ALPERT. New York, Teachers College Contributions to Education No. 323, Columbia University, 1928. Pp. viii, 69.

The author of this study has adapted Köhler's well-known problems for chimpanzees to children. Experimenting with forty-four children, whose ages ranged from 19 to 49 months, she devised two series of tests, as follows:

Series I. (A) A toy was suspended from the ceiling which reached within four feet of the floor. Five feet away diagonally to the right was placed a hollow block, nine by twelve by fifteen inches, which could be used as a foot-stool for reaching the toy. In (B) a chair was substituted for the block. In (C) the objective was placed on a shelf, four feet above the floor, with the block present for use as a foot-stool. (D) was like (A), except that the objective was hung higher, and could be reached only if the block were placed on end. In (E) the objective was hung still higher, and could be reached only after the block had been placed on top of a larger box which was now placed in the room.

Series II. (A) The child was placed in a play-pen, the objective being outside, and a stick about a yard long inside. In (B) a toy broom replaced the stick. In (C) the stick was now placed outside the pen, but it could be reached by a shorter stick which was inside the pen. In (D), in order to reach the objective two halves of a fishing rod must be joined together and used as a single stick.

Only four of the forty-four children made no attempt to solve the problems. Direct responses on the part of those who did make the attempt were about twice as frequent as indirect responses. There was practically no correlation between success in the solution of these problems and the mental ages of the children, thus confirming a general observation that social and emotional immaturity are the most likely reasons for failure.

An analysis of the behavior shows four types of response: (1) A primitive response, like reaching with the hand. (2) A random response which seems to be largely attributable to resentment at being thwarted. (3) Exploration and elimination of unsuccessful means. (4) An immediate solution which may include a brief primitive response.

After the first success the primitive response becomes infrequent. There were fewer random responses in Series I than there were in Series II. The subject does not adhere to one type of response but varies with the needs of the situation. Exploration and elimination appeared most frequently of all, with primitive responses a close second, while random responses were relatively infrequent.

The analysis of the solutions indicates "insight" rather than "a fortuitous constellation of elements." "If this whole question is regarded for the time being outside the setting of theoretical implications, the assumption that the solutions of the present problem-situations were accompanied by insight is not only warranted but inevitable and even obvious. What is not obvious is how insight evolved, and this appears to the writer to be the crux of the problem" (p. 38). The solutions are classified, first, as solutions with immediate insight; second, as solutions with gradual insight, which may be either partial or complete; and, third, as solutions with sudden insight which may mature during the exposure or between exposures to the same problem.

With respect to transfer, the author finds it to be almost perfect in Series I from A to B and C, and in Series II from A to B. Sixty out of seventy-one possible solutions in these experiments were immediate and are accountable in this way. Of the eleven cases showing absence of immediate solution in I-B, I-C, and II-B, six solved the key-situation with only a partial insight, and the other five can be adequately explained on other grounds, which makes "a fairly strong case for transfer as a criterion of insight" (p. 45).

There were no "chance solutions," although it may be said "that chance may be instrumental in bringing about an optimum constellation of the elements of a problem-situation, and that this constellation may or may not arouse insight. But unless insight is aroused, the most favorable constellation may go astray and the problem remain unsolved" (p. 50).

There follows a comparative study of the behavior of children and apes in these tests, and an analysis of the various causes for failure to solve the problems.

Cornell University R. M. OGDEN

Psychology and the Soldier. By F. C. BARTLETT. Cambridge, Cambridge University Press, 1927. Pp. viii, 224.

This book is a selection from certain lectures on the relations of psychology to military problems, given by the author in Cambridge. The treatment is somewhat general, dealing with principles, and is too abstract for most immediately practical purposes. Its three parts deal respectively with Choosing and Training the Recruit; Leadership, Discipline and Morale; and Mental Disorders of Warfare. Emphasis is put on the changes in psychology from speculation about the contents of the mind to the study of how people behave and why they behave as they do. Tests of visual acuity, the American army intelligence tests, and certain tests of special abilities are discussed as to nature and purpose. A few well known and experimentally established principles of learning are discussed in relation to the acquirement of bodily skill. It is pointed out that periods showing plateaus in learning should not be allowed to discourage one, and the Bryan-Harter interpretation of the plateau is offered. Persistent and prolonged repetition is discouraged as being related to becoming "stale," and the benefits of properly distributed practice are urged. An experiment on typewriting is described which led to the conclusion that stress on speed with neglect of accuracy at first and stress on accuracy to the neglect of speed, come out, when the stresses are shifted later to the neglected aspects of practice in each case, very much alike. The conclusion was supported that "a high quality of work can be obtained finally without insisting upon a high quality constantly throughout the learning process," but it is, of course, not regarded as final and of universal application. This ready change from accuracy to speed, and contrariwise from speed to accuracy, is held to be important. Valuable suggestions for motivation are offered, and the author advises against unvarying uniformity in practices. He suggests careful study of the effects of factors like shiftings, different rhythms, durations of practice, and holds that "nobody has as yet produced unequivocal evidence for the occurrence of direct mental fatigue" (p. 90). Mental fatigue shows itself indirectly in various nervous and mental disturbances, which are considered in the latter part of the book. Group psychology, in relation to morale, discipline and punishment, is handled less experimentally, of course, than the foregoing topics, but nevertheless with judgment and discrimination; and the author indicates various means of building up morale and shows that officers who are responsible for the conduct of court-martial should have some acquaintance with modern psychology. An interesting and valuable discussion of three types of leaders is given—the *institutional* type of leader, who must emphasize his rank and authority, the *dominant* type, and the *persuasive* type. "The institutional leader *must* remain aloof; the dominant leader *may* remain aloof; the persuasive leader *dare not* remain aloof" (p. 148). The last part of the book takes up (1) "the course of a soldier's entirely normal reactions to modern warfare," (2) the origin and nature of the symptoms of conversion hysteria and anxiety neurosis, two of the greatest classes of mental troubles developing in conditions of modern warfare, and (3) methods of treatment. These discussions are based on general experiences in the World War. It is, of course, hard to say just what the normal soldier's experiences are and there are differences of opinion concerning many details as to the on-set and real nature of the mental troubles. The author is not unaware of these facts and shows the caution of the scholar. Throughout the book he makes the reader realize that we are on new ground in which only a beginning of exploration has been made, and he emphasizes the need of adequate facilities, supplied by military authorities, for the prosecution of psychological research in the preparation of military careers.

J. P.

The Scientific Habit of Thought. By FREDERICK BARRY. New York, Columbia University Press, 1927. Pp. xiii, 358.

In four chapters—Science and the Sciences, The Nature of Fact, The Elements of Theory, and Scientific Humanism: A Corollary—the author attempts, apparently both as chemist and as philosopher, "to impart to the people at large the living spirit which animates (the scientist's) labor, the instinctive [sic] philosophy which guides it, or the methods which make it effective." The book is the substance of casual notes "taken down at intervals during four years of collegiate teaching in the history of sciences." While to the onlooker science is knowledge, to the scientist as worker it is activity; the spectators see the game, but the players live it. The particular methods of ascertaining facts vary in the different sciences with the contents investigated, but the general attitude of the investigators is similar. The results of one scientific activity provide the methods of another, and "the wonder of knowledge is not the richness of its present revelations but the limitless possibilities of its self-fertilizing growth." All "tough-minded" persons are scientific but not scientists, the latter only being those who make the search for new knowledge a profession. Scientists are the most reticent and inconspicuous, in the popular view, of the designers of our civilization. They are objective, detached, imperturbable, coldly and even recklessly rational. To them it matters little whether facts are beautiful or ugly, useful or not; they are not to be selected for private ends—such handling of facts is not science. But these are their attitudes only as scientists. In fact "all people are tender-minded part of the time, the larger number a greater part of the time, many all the time; this is their common nature, and in this their brotherhood, which is real, consists." The characteristic traits of the tough-minded are only adaptations, liable to revert and show instabilities. Scientists appear to "find their greatest satisfaction not in the indulgence of their emotions or aspirations but in the prosecution of affairs." The gratification of curiosity is stressed too much by the author as the motivation in science. Curiosity is often but a superficial drive.

The valuable discussions of the nature of fact and the elements of theory are well worth reading. The general attitude taken by the author is pragmatic, and reveals indebtedness to writers like John Dewey. The handling is unfortunately, due to its abstract and historical nature and the limitations of space, beyond the grasp of most general readers even of college grade; and the illustrations are too narrowly confined to the science of chemistry. The chapter on scientific humanism is hardly up to the level of the earlier part of the book. It considers the rôle of science in modern culture, and also methods of the in-differences in mental ability are apparent even during the first few weeks of life, and that these differences are maintained at approximately the same relative level from early infancy to maturity.

Following an introductory chapter, the book is divided into three parts. Part I deals with observational methods. A full description of the plan and equipment of the Psycho-Clinic and the Guidance Nursery is presented, together with many photographs. An extremely valuable feature of this part has to do with the method of constructing an observational screen for one-way vision, and a photographic dome with electrically controlled cameras which provides for the exact synchronization of simultaneous photographs taken from different angles (described also in this JOURNAL, 40, 1928, 126-128).

In Chapter V the method of studying developmental changes by simultaneous comparison of the behavior of two children of different ages, which was first described in his earlier work, is again brought into play. The paired subjects range in age from 1 to 12 mo., and each successive comparison is made between children who differ in age by only a single month. Chapter VI summarizes the various normative items which have been found typical of children at each monthly age-level from 1 to 12 mo., and thereafter by 3 mo. intervals to 30 months. According to his usual custom, the author groups these items under four main heads: motor, language, adaptive, and personal-social behavior.

Part II includes a group of special genetic studies of infant behavior. In Chapter VII Gesell discusses the form of the mental growth curve, and the consistency of the "developmental quotients" (identical in derivation with the familiar "intelligence quotient"), obtained through repeated examinations according to his methods. This chapter is open to definite criticism. The consistency of the reported findings for the same children from examination to examination is almost unbelievable until one notes the statement (p. 141) that "the repeated examinations were made by the same examiners, as it was our purpose to build up a cumulative familiarity with the child. In each instance, therefore, the last examination was brought into comparison with the results of the preceding examination. A new series of ratings were made and *the data on the previous examination were used critically in the weighting of the succeeding examination*" (italics mine). When the highly subjective nature of many of the individual normative items is taken into consideration, one can hardly avoid wondering how much of the reported consistency of results would remain if the usual scientific requirements of independent observation and judgment had been adhered to.

The remainder of Part II is given up to a series of studies on special aspects of mental growth. The case study method is used throughout. Normal, retarded, and accelerated children are compared as to development during the period of infancy. There are a number of interesting reports on cases showing atypical glandular and nutritional conditions. A chapter on twinning includes an account of the mental development of two cases of hemi-hypertrophy, which the author, in common with many others, regards as a form of minimal twinning. A description of the mental development of premature and postmature infants presents some data to indicate that, as Scammon has demonstrated in regard to physical growth, the normal development cycle is but little affected by the incident of birth. Conception, rather than birth, should be regarded as the zero point in computing indices of development.

Part III, "The Significance of Infancy", presents some comparative data on human and animal infancy, discusses the relative influence of hereditary and environmental factors, the possibilities of prediction, and the social importance of early prediction, particularly in cases of adoption.

Taken as a whole, the book is highly suggestive. Many problems are pointed out, and certain outstanding trends of mental growth during the period of infancy are described. Those who have been inclined to regret that the emphasis upon end-results presented both in this and the preceding book has led to rather meager descriptions of the methods by which these results have been arrrived at, will be particularly gratified at the promise of a detailed manual giving complete descriptions of procedure together with diagnostic norms. We shall look forward to the publication of this manual with much interest.

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Psychology in Personal Selling. By A. J. SNOW. Chicago, A. W. Shaw Co., 1926. Pp. xvl, 620.

In late years there has been a great emphasis by certain writers on the 'psychology' of salesmanship. Evidently because the seller is better organized and financially more able to reward such services than are the individual buyers, the writers are more ready to help him with his aspect of the double process of selling and buying. We do not yet see books on how to avoid buying what is of relatively little value to one, or even on the psychology of individual buying. While there is no denying that advertizing and selling play important rôles in the general education of the public and in the standardising of products, it is equally undeniable that there are many so-called psychologies of these functions which are no credit to psychology; indeed, they are mostly books by popular writers who know very little of psychology and who care less for scientific standards. The author of this book has formerly attempted in his *Psychology of Business Relations* to emphasize the fact that " a good sales situation has three elements, (a) a real need, want, or desire, (b) a solution, and (c) satisfaction."

The present book is divided into four parts: (1) Psychology of Personal Selling; (2) The Salesman; (3) The Sale; (4) Selection and Training of Salesmen. The author first attempts to give an outline of reflexes and their modifications; motives to action, to fear, to fight, to conceal, to shelter, to own, to assert oneself, to be submissive, to know, to imitate etc.; individual differences; perception; habit; ideation; etc. Other conventional topics come in under "the sale." The author follows the fashion and is a 'denier' of instincts (of the incredible armchair type of 'instincts') and yet his treatment of the "desires, wants, needs" etc. just given shows that the departure from the conventional is only superficial. While much of this conventional treatment seems to the reviewer somewhat valueless and far-fetched (probably for any book on psychology), considerable objective data valuable to the business man are included, though unfortunately they are not always highly reliable. The high intercorrelations among personal traits as estimated, for example, by friends and the value of objective studies designed to clarify the learning process, but the presentation of these studies alone seems inadequate in dealing with learning. The educational psychologist cannot escape the necessity of formulating principles; for these principles are essential if educational theory and practice are to profit from psychological investigations. This is particularly true, it seems, with respect to the stages of learning, the learning process, and the patterns of learning. In this connection, the author is content to quote Thorndike's laws (use, disuse, satisfactions, bonds, etc.), although it has been pointed out by others that these are perhaps factors affecting learning rather than laws of the nature of the learning process itself.

If, however, one accepts the author's premise that the subject matter of Educational Psychology should be restricted to the presentation of purely objective and verifiable data, it will be found that the volume has fulfilled its purpose by the richness of its material. The details and data of many experiments are cited with reference to such problems as conditioned stimuli and conditioned reactions, laws of and improvement in learning, permanence of improvement, fatigue in learning, and transfer of training. There are also brief chapters dealing with the measurement of intelligence and with the measurement of achievement by means of the new type, or objective, test. Throughout these chapters, it seems, it has been the author's purpose chiefly to indicate the nature of the experimental data, rather than to develop principles from them.

Cornell University FRANK S. FREEMAN

An Approach to the Psychology of Religion. By J. CYRIL FLOWER. New York, Harcourt, Brace & Co., 1927. Pp. xi, 242.

The author's fundamental thesis is that religious phenomena are connected with original tendencies "not directly, as immediate, distorted or sublimated expressions of their operations either singly or in cooperation, but indirectly, as the result of their failure to function in an ever-increasing discrimination of features in the environment," a discrimination altogether in excess of man's innate tendencies for direct response, and which gives rise to what is designated as the "frustration experience." He assumes that "beyond situations" give rise to phantasies and images which are projected upon them in two main ways: (a) phantasy as a means of practical control, (b) phantasy as a means of partial substitution. The first gives rise to scientific knowledge, the second to religious faith. A distinction is made between imaginative interpretation and phantastic distortion. One is practical and adaptive, the other impractical and mal-adaptive. Herein lies the essential difference between religion and insanity.

Later chapters make applications of the above theory, as an explanatory principle, to the religion of the Winnebago Indians, the peyote cult among the Winnebago Indians, and the religious experiences of George Fox. The source material is purposely selected to represent religious experience at different cultural levels, and in each instance the author implies that a satisfactory explanation has been achieved. Some of the salient features of the conversion experience are explained as frustration phenomena, and as the most frequent particular instances of "the first hand religious response." The author criticizes the views of Jung, Martin, etc. as being unduly depreciatory of religion. Religious phantasy is not merely an escape from a reality which is displeasing, it is a construction about a reality which is baffling. It is difficult to evaluate empirically in a reliable manner the interesting hypotheses of the author.

University of Georgia JAMES E. GREENE

The Child Guidance Clinic and the Community. By R. P. TRUITT, L. G. LOWREY, C. W. HOFFMAN, W. L. CONNOR, E. TAYLOR, and F. R. KENDEL. New York, The Commonwealth Fund Division of Publications, 1928. Pp. 106.

This little book consists of a group of papers written from a number of points of view describing the function of the child guidance clinic in the community. Dr. Truitt presents a brief outline of the history of these clinics, and dwells particularly on their function as centers for community education in mental health. Dr. Lowrey outlines the general plan of organization, and describes in some detail the various kinds of services rendered. Judge Hoffman points out the relationship of the clinic to the juvenile court, and gives a summary of the results of preventive work with juvenile delinquents in Cincinnati, together with several case histories. Mr. Connor points out the services which the clinic may render to the public schools. Miss Taylor deals with its services to children's agencies, and Mrs. Kendel dwells upon its functions as a means of bringing about better understanding between parents and children in the home.

A foreword by Graham Romeyn Taylor points out the changing conception as to the function of these clinics. "Attention was naturally focussed first on the value of the clinic itself, with its coördinated service of psychiatrists, psychologists, and social workers, to the children concerned." "More recently, however, members of the various professional groups and laymen interested in this service to children have become aware that its effectiveness depends largely upon community understanding of the clinic's function, and the proper adjustment of the relationships between the clinic and the various agencies with which it coöperates.

Institute of Child Welfare
University of Minnesota FLORENCE L. GOODENOUGH

The Nature of Conduct. By PERCIVAL M. SYMONDS. New York, The Macmillan Company, 1928. Pp. 346.

This volume is an interesting and suggestive discussion of a significant educational problem. Conduct, according to the author, is "that overt behavior of a human being which involves an issue respecting himself, that issue making a difference in his satisfaction with life." Stated more specifically, the writer is interested in the essential psychological nature and the development of such complex forms of reactions as courage, truthfulness, etc.

Phenomena of this sort are "the habits and skills with which we meet life situations." They are not to be regarded as unitary traits but each is typically a group of relatively independent habits. Also, the component habits are responses to definite stimuli and such loose unity as may exist is due primarily to a stimulus factor common to the several stimulating situations.

There are several very effective features in the book. A chapter on the social anthropology of norms of conduct and another on the place of reasoning in conduct are excellent. Also, through the extensive citation of experimental literature, several important aspects of the general problem are made clear.

The author undertakes to treat his subject from the point of view of behaviorism. Although he presents the behavioristic concepts very effectively, he does not hold to them with perfect consistency. A discussion of the rôle of the affective processes involves introspective data almost exclusively. The concept of purpose is also introduced in the final chapter.

Dr. Symonds is seriously confused about one point. He says that the problem of ideo-motor activity is, "Do ideas have control over overt conduct?"

This volume can hardly serve as the sole text of a course in elementary educational psychology because of its restricted scope. As supplementary reading, it will serve to correct a host of misconceptions.

University of North Carolina ENGLISH BAGBY

The Cave Man's Legacy. By E. HANBURY HANKIN. New York, E. P. Dutton and Co., 1928. Pp. 180.

Somewhere between the anthropoids and the most primitive human life known there must have been a stage in our ancestry when men were extraordinary brutal. During the known history of man this brutality has slowly subsided along with the development of morality, but it remains in us as an instinctive tendency which is often manifested. This is the gist of the author's thesis. The scope of his topic is notable. From the first chapter in which there is an animated presentation of his own ability to communicate with monkeys, the author leads his reader through primitive pugnacity, murder as a habit, cannibalism, evolution of the human mind, the origin of morality, events of the French revolution, the Paris Commune and the American revolution.

One would think that the author had never heard of John Watson and the critical literature of instinct theory. He speaks glibly of the instinct to kiss, sexual jealousy as an inheritance from animal ancestors, love of dancing, the property instinct, the stranger-hating instinct (the instinctive desire to kill a stranger on sight, of which race prejudice is a vestigial form), instinctive cruelty, a natural tendency to sympathize with aspirations for home rule, and perhaps others. There is, curiously, no use made of G. Stanley Hall's more scholarly presentation of a very similar thesis concerning an assumed exterminating tendency in primitive man. Such a book as this is very likely to give the reading public a distorted notion of what psychologists are thinking.

University of Oregon EDMUND S. CONKLIN

The Psychology of Youth. By E. J. SWIFT. New York, Charles Scribner's Sons, 1927. Pp. xi, 342.

This volume represents a new edition of Professor Swift's book, *Youth and the Race,* published in 1912. The publication of this revised edition is very timely in view of the rapid increase of freedom gained by the adolescent youth since the World War. Concerning the 'Flaming Youth' of today, Professor Swift says, "Perhaps the 'revolt' is no greater than in the past, but with a wider range of opportunities it assumes more alarming proportions." After presenting many up-to-date examples of adolescent difficulties a solution similar to that of the earlier book is offered. The author concludes: "Definite rules of action for tapping the reservoirs of racial and social energy cannot be given. It is largely a matter of personality and tact in dealing with children. But the first requirement in the teacher is to remove the coating of pedagogical tradition. Then he is ready to absorb modern ideas. If such a teacher determines to find a plan which will create in the children the same enthusiasm for school work as they have for their own activities, he will know when he has succeeded."

Princeton University W. S. Hulin

Orleans Algebra Prognosis Test. By J. B. and J. S. Orleans. Yonkers, N. Y. The World Book Co., 1928. Pp. 14.

As the title indicates, this test is intended to predict a student's chances for successful work in high school algebra. It examines the student's ability to handle situations such as he must meet in the study of algebra, and also his knowledge of arithmetic and other information necessary for learning algebra by methods now in common use, and his skill in applying such information. Character traits—such as attention, persistence, interest—are regarded as being an important factor in algebra achievement, for some of the students used in the standardization process were graded on the New York Rating Scale for School Habits. The multiple correlation for the prognosis test and the rating scale, on the one hand, with achievement test scores on the other, was 0.82. For the prognosis test and achievement scores the multiple correlation was 0.71. The achievement scores were taken after one term in the study of algebra. The authors do not state whether the prognosis test is to be regarded as a criterion of the quality of work at any level in secondary school algebra, or whether it is valuable chiefly or only for the most elementary levels.

Cornell University Frank S. Freeman

Objective Tests. By J. S. Orleans and G. A. Sealy. Yonkers, N. Y., World Book Co., 1928. Pp. x, 373.

This book presents a plan for the local construction and use of objective school tests for instructional, administrative, and supervisory ends. It is also intended for the use of teachers and school officers in a course of training. The subject is presented through a detailed description of a testing program carried out in one of the supervisory districts of Lewis County, N. Y. The volume deals but very little with the general theory of standardized tests; it is devoted rather to the problem of local needs and their possible satisfaction through the coöperative work of teachers and school officers. The authors have included sample tests in a number of subjects and sample record sheets for the assistance of those who will use the book as a guide. The authors have been careful in the presentation of their material and modest in their claims for the tests and programs which they have outlined.

Cornell University Frank S. Freeman

NOTES AND DISCUSSIONS

ENERGY, ENGINES, AND THE ENGINEER

A CRITIQUE OF C. SPEARMAN*

Professor Spearman's *The Abilities of Men*[1] can hardly fail to be reckoned among the few masterpieces of expository writing in psychology. There is not an obscure sentence in it, and by generous use of summaries and recapitulations the essential points are driven home. His long-known doctrine of '*g*,' the general factor in intellectual abilities, is now fully before us. Its proof rests upon the 'tetrad difference' formula, the argument being that given four tests each measuring a different mental function, if the difference between the product of any two correlations and the product of the other two correlations approaches zero, the functions tested must involve a single general factor and four specific factors. This condition Spearman finds fulfilled by a large variety of mental tests. He supplies the essential need of a formula for the probable error of the tetrad difference, and measures the amount of the general factor involved in any mental function tested, by an ingenious use of the formula for partial correlation.

Discussion of the statistical validity of these methods may be left to the experts in that field. The present paper is concerned rather with Spearman's conclusions as to the nature of the factors which he discovers through analysis of a large mass of experimental material.

These factors are the following. In the field of cognitive processes three general factors are revealed by the tetrad difference method. The first is Spearman's original general ability, '*g*,' which appears in tests involving 'eduction,' or the cognition of relations and correlates; for example, in the opposites test and in various reasoning tests. The second general factor is relative absence of perseveration, as shown by the ease with which a person can pass from one task to another. The third is oscillation of efficiency, as seen in rivalry, fluctuations in the limen, and continuous mental work. There are also four less general or group factors, manifested in certain groups of tests; these are logical, mechanical, psychological (apparently meaning ability to interpret other minds, to deal with human nature), and arithmetical. In the field of conative processes, Spearman accepts as a general factor Webb's '*w*;'[2] self-control, or purposive consistency, "a tendency to take pains in the present for the sake of gains in the future" (p. 354).

Spearman's interpretation of these factors relates them to mental energy, to the engines through which that energy works, and to a mysterious engineer. In the first place, the general factor *g* is supposed to be the quantity of mental energy possessed by an individual. Perseveration represents the degree of inertia affecting this energy, and oscillation indicates the rapidity of its fatigue and recovery.

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*Read at the meeting of the American Psychological Association, Columbia University, December 27, 1928.

[1] C. Spearman, *The Abilities of Men*, 1927.

[2] E. Webb, Character and intelligence, *Brit. J. Psychol., Mon. Suppl.*, 1, 1915, no. 3, 1-99.

In the second place, the group factors and the specific factors are held to depend not on the supply of mental energy, but on the efficiency of various particular engines, peripheral or central; thus the specific musical ability may depend on peculiarities of the ear and of the auditory region of the cortex.

Thirdly, as for the conative general factor, character or self-control, "some of us," says Spearman, "may be inclined to take yet another step and think that where energy and engines operate there must furthermore exist an engineer. And this requirement also seems to be met, namely, in the conative law—as specially manifested in *w*" (p. 415).

The present writer objects to these interpretations in two particulars. First, to the vitalistic assumption of an engineer. Secondly, to the assumption of a mental energy whose laws do not harmonize with those of nervous energy.

As regards the first point, the hypothesis of an engineer who is not himself an engine may, I think, be regarded as a product of that tendency to mysticism in biological speculation which seems to have followed the war as a kind of fatigue phenomenon: whenever it is not perfectly simple to explain a biological fact by ordinary physical laws, the easy way is to conceive the activity of a super-physical being. I have elsewhere[3] suggested a possible mechanistic basis for directed thinking and that self-control which constitutes Webb's *w* and Spearman's engineer, and will not discuss this matter further here. The main concern of the present paper is with the second point, the assumption of a mental energy whose laws do not harmonize with those of nervous energy. I wish to show that this lack of harmony arises wholly from Spearman's error in conceiving his general cognitive factor, *g*, as due to *quantity* of mental energy, instead of thinking of it as dependent on a certain structure of the central cognitive *engine*, the cortex.

To begin with, let us see how Spearman conceives the relation of mental energy to nervous energy. He would like to think of the two as identical. He quotes as expressing "a promising idea" Myers's statement, "I see no reason why we should not identify central nervous energy with mental energy" (p. 407). But for the present he admits that the two forms of energy do not coincide. The conception of mental energy is, he says, "incomparably less definite than that of the physical energy which serves the physiologists so well." As for the relation between the two things, this, he continues, is for the present "buried in complete obscurity."

Just what the incompatibilities between the two types of energy are Professor Spearman nowhere concisely states; we can, however, discover them by a careful consideration of what he has to tell us about mental energy.

The most striking incompatibility seems to lie in the fact that individuals with most nervous energy, so far as we know anything about nervous energy, seem not to be those individuals who have the greatest quantity of mental energy, of *g*, general cognitive ability. For example, a large stock of nervous energy is put at the disposal of the organism by the action of the endocrine glands. But the person whose endocrine glands function most energetically, the emotional person, is not apt to be the person in whom we find the most cognitive ability. Moreover, it is often precisely when the endocrine glands are most vigorously acting that the processes which manifest g, the processes of relational or eductive thinking, go out of business.

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[3] M. F. Washburn, Purposive action, *Science*, n.s. 67, 1928, 24-27.

Shall it be said that the person with most mental energy is the person in whom the greatest proportion of available energy discharges not in the thalamic region, to produce the manifestations of emotion, but in the cortex, to produce thought processes? But not only does g fail to correlate with quantity of energy released by the endocrine system; we have no assurance that it correlates with the quantity of energy used in cortical processes. Memory, at least in its higher types, is in all probability a cortical function; and one of Spearman's most striking discoveries by the tetrad difference method is that g does not correlate either with retention or with recall. Reproductive processes involve no g. While memory weakens with age, he finds no corresponding diminution in g. It is in the processes that Spearman calls *eductive*, the cognition of relations and correlatives, and in these alone, that g is shown. But to assert that eduction requires more mental energy than does reproduction is merely to beg the question.

Surely the peculiar nature of g which causes it to be exercised in eduction but not in reproduction constitutes a disharmony between mental and nervous energy that may be removed by merely supposing that g correlates not with quantity of mental energy but with a peculiarity of the cortex as engine. On the mental side the difference between reproduction and eduction is that reproduction need not involve analysis of situations, while eduction always does; to discover relations we must analyze. What this analysis means on the cortical side we do not know. A motor theory would have it involve the power of making many combinations and recombinations of many different small and delicate movements. Perhaps "delicacy of response" is as good a name as any for that trait of the cortex as an engine which must characterize the brain of a person in whom Spearman's g is large. It is not quantity of nervous energy, which may be larger in a stupid but emotional person; not even quantity of nervous energy in cortical processes, for this may for all we know be as high in the person whose superiority lies in memory as it is in the thinker; but a certain capacity for detail reactions rather than mass reactions, a structure innate in the cortex of such individuals as possess keen reasoning power, the 'electric aptitude for seizing analogies,' and lacking in the cortex of those dull persons who reproduce their previous responses *en masse* and without recombinations.

Spearman himself suggested in 1906[4] 'plasticity' or 'plastic function' of the nervous system as the correlative of g, and mentions the fact in *The Abilities of Men* (p. 91), apparently as a matter of merely historical interest. Why does he now adopt the so much less appropriate and so much more mysterious concept of quantity of mental energy? Curiously enough, his strongest reason appears when looked at more carefully to indicate that g rests *not* on quantity of energy, but on plasticity or something like it, of structure in the cortex, the general engine.

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[4] F. Krueger und C. Spearman, Die Korrelation zwischen verschiedenen geistigen Leistungsfähigkeiten, *Zeit. f. Psychol.*, 44, 1907, 50-117.

The reason lies in what he calls the law of universal mental competition or of span: "every mind tends to keep its total simultaneous output constant in quantity, however varying in quality" (p. 259). There is, in other words, a sharp limit to the number of mental processes that can occur simultaneously. When a group of beans is exposed to view for a very short time, "*any* two may both be seen clearly . . . so long as not too many others are seen clearly also. . . . Similarly, one can easily follow any two instruments in an orchestra" (p. 114). It is this limitation of output, suggesting as it does the law of the conservation of physical energy, which seems to be the fact that influences Spearman to think of g as quantity of mental energy rather than as due to a peculiarity of structure in the engine.

But obviously in order to conclude from the limitation of mental span to the limitation of mental energy as shown in g, it must be shown that size of mental span as such correlates with g. One is astonished to find from Spearman's data that it does not so correlate *as such* at all. The following are his data. In tachistoscopic experiments with verbal material, children previously estimated as more intelligent showed larger spans; but as Spearman says, this may have been because the more intelligent children had learned to read better. When non-verbal material was used, the correlations with g were low. Only one research, that of McQueen,[5] in which the observer was required to perform two mental operations at once, is regarded by Spearman as trustworthy; here the correlations with g were satisfactory. But Spearman himself points out that in such cases there is often rapid alternation between the tasks instead of simultaneous performance, and—here is the point at issue—that when they are simultaneously performed, it is usually or at least "much more naturally" *when there is cognition of the relations* between the two tasks (pages 264-5). Obviously this means that mental span as such does not correlate with g at all; the observer with the greatest span is not the observer with most g *unless the processes involve eduction.* It is not mental span but eduction, the cognition of relations, with which g correlates; the law of mental span then can furnish no evidence that g is quantity of mental energy. The only trustworthy experiments point rather to the conclusion that g is an innate characteristic of the general engine, the cortex, by virtue of which analysis and response to relations become possible.

As for the other two general factors which the tetrad difference formula discovers in cognitive processes, Professor Spearman suggests that one, oscillation in efficiency, is due to a peculiarity of the central energy, namely, the speed with which it recovers from fatigue. "Such an account," he says, "of the way in which fatigue is manifested by the general energy furnishes the needed complement to the preceding chapter, where an account was given of how fatigue is manifested by the specific *engines*" (page 327, italics mine). The other general cognitive factor, the perseverative tendency, he explains as due to the degree of inertia belonging to the individual's mental energy, which, he says, "is shown in slowness of shifting from one set of engines to another." But surely fatigue and inertia belong to engines rather than to energy?

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[5] E. N. McQueen, The distribution of intelligence, *Brit. J. Psychol., Mon. Suppl.*, 1, 1917, no. 5.

In comparison with the solid bulk of Spearman's achievement in analyzing and measuring the energies of men, it may seem an unimportant matter to criticize his interpretation of *g*. My interest in it arises from two sources: first, the obvious wrongness of the interpretation, and secondly, a feeling that the assumption of a mental energy whose laws are inconsistent with those of physical energy arises from the same mystical tendency that assumes the control of an engineer. We all inherit an engine which secures for each of us a certain available supply of physical energy; this engine is our metabolic apparatus, including the endocrine glands. The amount of energy each man's metabolic processes can supply seems not directly correlated with the power of his intellect. Shall we conclude that we all inherit also a certain quantum of a type of energy not derived from metabolism, and that our intellectual powers depend on this super-physical energy? Or shall we suppose that each of us inherits a central engine, the brain cortex, whose peculiarities of structure determine his intellectual ability by determining what proportion of the energy derived from metabolism can be used for thought? Not innate quantity of mental energy, nor the power of a super-physical engineer, but innate differences in the structure of the engine, seem to me the most probable basis for differences in the abilities of men.

Vassar College M. F. WASHBURN

EIGHT LETTERS FROM G. STANLEY HALL TO H. P. BOWDITCH
WITH INTRODUCTION AND NOTES

In 1871, Bowditch and Hall, two outstanding American scholars whose names are often joined, had not met each other. One was a physiologist and at that time the other was a philosopher. In the year mentioned they both returned to the United States after having enjoyed prolonged periods of study in German universities. Bowditch was 31 years of age, he had proved himself a capable student in Professor Ludwig's famous laboratory of physiology at Leipzig; and he had just married Miss Selma Knauth of that city. He was returning bag and baggage (the latter containing well selected physiological apparatus) to accept a position as assistant professor of physiology at the Harvard Medical School and was soon to establish the "first physiological laboratory for the use of students in the United States."[2] Professor and Mrs. Bowditch ever maintained an especially close contact with German physiological science. Their home was a natural objective for many visitors (including Helmholtz) from abroad and many Americans going abroad had the way paved for them by the Bowditches. Hall, six years younger than Bowditch, had studied philosophical theology under Dorner in Berlin. He had become acquainted with Von Hartmann and other German philosophers and had done

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[1] These letters were placed at our disposal by Miss Frances N. A. Whitman, Librarian of the Harvard Medical School.

[2] W. B. Cannon, Henry Pickering Bowditch, *Mem. Nat. Acad. Sci.*, 17, 1924, 183-196.

some work as war correspondent at the outbreak of the Franco-Prussian struggle. Returning to New York he became for a second time a student at Union Theological Seminary and a few months later received the B.D. degree. There is no evidence that Bowditch and Hall knew each other prior to about 1876.

Hall, as is well known, taught English, modern languages and several other subjects at Antioch College from the fall of 1872 to the spring of 1876, *i.e.* for four academic years. He tried to resign in the spring of 1875 as he wished to go again to Germany, this time to study with Wundt, whose *Grundzüge der physiologischen Psychologie* had appeared in 1874, but the Antioch College Board would not accept his resignation.[3] When Hall finally left Antioch in 1876 he apparently had definite plans for going immediately to Wundt at Leipzig.[4] From these he was deflected for a period of two years, during which he occupied a tutorship in English at Harvard. Of this period Wilson says,[5] "in spite of the large amount of required work [reading and correcting Sophomore papers], in which he took but the slightest interest, he found time to attend courses under Dr. H. P. Bowditch at the Medical School on Boylston street, and to work in his physiological laboratory on "The Muscular Perception of Space," which he presented as a thesis for the Doctorate in Philosophy in June, 1878.[6]

Just what circumstances brought Hall to the physiological laboratory we do not know. His reading of Wundt may easily have generated this interest. Bowditch was at this time the center of a very live group of workers. Certainly Hall and Bowditch had a strong common bond in their experiences and acquaintances in Germany. Just how much more it would have taken to swing Hall definitely and forever into physiology we can not judge. Apparently he was not far from that decision several times during the next few years. That his thinking and interests were greatly stimulated by his contact with Professor Bowditch is clear to see. Bowditch was carrying on the study of the growth of children before Hall left for Leipzig. Hall was later to refer to him as the "father of child study." Bowditch urged Hall to go direct to Ludwig, and Hall later wished that he had done so.

The four letters before us written by Hall from Germany give an interesting cross-section of what was going on fifty years ago in an important scientific period. Hall was keen for keeping in touch with every thing, he knew that Bowditch was keen to hear about everything; conditions were thus right for letters full of informing details.[7]

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[3] Louis N. Wilson, *G. Stanley Hall*, 1914, 58.

[4] In Hall's *Life and Confessions*, he states (p. 204) that he did go to Germany in 1876, but the account by Wilson seems clear and trustworthy, also in the particular that Hall completed his thesis on "The muscular perception of space" with Bowditch in 1878 and got his Ph.D. from Harvard prior to this second trip to Germany, whereas from the *Confessions* (pp. 204-219) one gets the impression that the Ph.D came after the study with Wundt and Ludwig.

[5] Wilson, *op. cit.*, 64.

[6] G. Stanley Hall, The muscular perception of space, *Mind*, 3, 1878, 433-450. This publication bears little if any internal evidence of experimentation and there is no statement that it was prepared under the direction of Bowditch.

[7] We have transcribed the letters as accurately as possible, preserving Hall's spelling wherever it is clearly decipherable. Because of Hall's indifference to legibility as well as to orthographic exactness it has not always been possible to identify definitely certain proper names and a few other words.

with valuable results on the nature of the precautions to be observed, and the subtlety of sensation in hysteroid subjects, but with no trace or shadow thus far of any thought-transference in the sense of the English Society.

I am mortified about your books which I have had so long, and if you are not using these I'd like them a *few weeks* longer as the work is now actively going on which we began.

Very sincerely yours,
G. Stanley Hall

I wish I could know what you think of the whole business, I feel about like withdrawing from the society, but hope you will not, for I sh'd certainly follow you.

Johns Hopkins University
Baltimore, April 19, 1885

Dear Dr. Bowditch:

My tastes, interest, reading etc. draw me more and more to make psychiatry, scientifically and not practically worked, the focus of my psychology, in which I did some work abroad including the courses of Westphal, Wermke and Meynert[40] and Flechsig, with whose writings I am acquainted with detail. If I had a degree of M.D. it would open here (and a summer or two I mean to spend abroad soon)[41] opportunities now closed but which I think I could utilize. For these reasons I venture to ask you, not as Dean but only as a friend for information and advice.

On the one hand, I think the practice of honorary degrees, particularly that of M.D. entirely and wholly bad. It is opposed to all my ideas of higher education. Yet on the other hand, the practice still exists and why should I not avail myself of its advantages for study. I should never practice of course, but use it only to procure entree to opportunities in that one line which seem to me the key of all psychological studies as well as educational ones.

Will you at your leisure have the goodness—for I know no one else to whom I can turn—to advise and instruct me.

Ever sincerely yours,
G. Stanley Hall

Clark University
Worcester, Mass.,
May 25th, 1889

My dear Dr. Bowditch:

Enclosed please find your V.[olume] which I return with thanks.

I wish you would let my Journal print some, or all, of those splendid things you are about to publish.[42]

The fact is it is very hard to get good original articles for the Journal, moreover, it does not pay expenses and I myself pay every cent of the deficit.

I feel that the fine print notices, which cost us a *great deal* of labor and which are made as complete as possible by *buying everything whatever* that seems likely to be good in that line and which is a private expense of considerable magnitude in addition to the deficit, are really an evidence of, at least an intention to render a real service. This makes me at any rate freer to ask men like you for articles like yours.

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[40] Th.H.Meynert (1833–1892), from 1870 professor of psychiatry and director of that division in the General Hospital in Vienna. See Hall, *Life and Confessions*, 215.

[41] Parenthesis added by the editors.

[42] Bowditch never published in the American Journal of Psychology, and only two of his articles are there reviewed (see I, 1888, 712 and 3, 1891, 577). For the bibliography of Bowditch see Cannon, *op. cit.*, 195-196.

Our rule is to pay for no reprints or plates, from the first number no exceptions have been made to this rule, but we are ready to make an exception at least for reprints and *possibly*, *possibly* for plates in your case.

Hoping very much I may be able to see you before you leave the country,

I am,

Ever most truly yours,
G. Stanley Hall

Clark University
Worcester, Mass.,
April 22, 1891

My dear Dr. Bowditch:

Dr. Boas has been subjected to violent attacks from a local paper here for wanting to measure school children. The paper has made many blind references and statements about your work, such as, that you sold the results of your measurements to hatters, clothiers, etc., that you have said they were of no use, were a failure, etc. etc. Of course we cannot reply nor pay any attention to anything on so low a plane, and you probably would not wish to do so, at the same time I should be very glad to have in my possession for future contingencies, with power to use or not at my discretion, a line or two from you covering these two points—whether you made money, and whether you ever said that your measurements were of no value.

If you felt like adding a line indicating what value they were for either Anthropology, Pedagogy, or anything else I should be very glad.

I hope to send you before long a few brief notes on the extremely interesting drawings.

I am, Ever Yours,
G. Stanley Hall

P.S. It is a ridiculous mess but with a few words from you as a reserve I may inspire courage. G.H.

The period covered by the last four letters, 1885-1891, was crammed full of professional work for both correspondents. It is possible in this work to trace a certain parallel of activity and interest. Bowditch was active in the founding of the American Physiological Society in 1887 and became its second president the following year. Similarly, Hall was prominent in the founding of the American Psychological Association accomplished in 1892 when he became its first president. From the establishment of the British Journal of Physiology in 1878, Bowditch was active as an American editor and contributor to this important channel for physiological literature.[43] Hall was of course a much more active editor. He established the American Journal of Psychology (1889) and as its first editor contributed both articles and reviews in large number. Bowditch continued to carry on laboratory studies and through this channel contributed to physiology, anthropology and pedagogical methods. Hall's work was not so clearly of a laboratory nature but was none the less fruitful and stimulating and covered a much wider range. Both men were inevitably crowded into administrative work, although they tenaciously held on to teaching and were both eminently successful in this capacity.

In the letters before us the younger man turns for guidance to his senior, almost as to his older self. Hall's intellectual inheritance was doubtless chiefly European but it is obvious from the present intimate glimpse that much had come to him through contact with a leader of American Physiology, whose words he apparently more than once counted on "to inspire courage" as well as to "advise and instruct."

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[43] Bowditch continued in this capacity with the Journal of Physiology until its 27th volume published in 1902. He was also one of a small group of Americans to inititate the American Journal of Physiology, January 1898.

Stanford University

W. R. Miles
C. C. Miles

A Protest

A review of Porteus and Babcock's *Temperament and Race* which appeared above the initials "J.P." in a recent issue of the Journal (Vol. 40, 1928, 640 f.) contains so many misapprehensions and misstatements on the part of the reviewer that I feel it calls for reply. In some respects the methods used and the conclusions reached by the authors are absolutely at variance with those attributed to them by the reviewer. The assertions that need correction are taken hereunder in serial order from the review.

(1) "Intelligence tests are included as measurements of temperament." This can only apply to the Maze tests which insofar as they measure a tendency to use foresight and prudence in the working of the tests, are tests of temperament.

(2) "A number of photographs of 'typical' boys and girls of the several 'races' are included. How 'typical' they are seems wholly a matter of personal judgment." On the contrary the children were selected because their measurements approximated most closely the average measurements of the whole racial group. This is indicated on the photographs as follows: "Typical 9, 12 14 year Japanese (or Chinese) boys approximating the average in racial head form." Hence 'personal judgment' did not enter into the selection at all. Further, the two photographs of 'typical' children in the book were boys and not 'boys and girls.'

(3) "Twenty-five judges (not described) are asked to estimate Japanese . . . as races according to planning capacity [and other traits]." If the reviewer had read the book carefully he would have found at the first mention of the judges a description of the group as follows: "Ratings were obtained from twenty-five people of long experience in the islands, observers whose work brought them into very intimate contact with the various racial groups. Sixteen of the judges were plantation managers while among the remainder of the group were head workers of social settlements, plantation doctors, and several educationists" (p. 90).

(4) "One is forced to ask how independently the judges acted of popular prejudices and of the bias of the authors." With regard to popular prejudices, it is, of course, impossible to say how much of these entered in, but the authors took pains to point out that in rating the Filipinos, Portuguese and Chinese in general, and the Japanese in aggressiveness, some prejudice might be expected. As regards freedom of the judgments from bias of the authors, we certainly did not think it necessary to state that the judgments were absolutely independently given, but took it for granted that we should be credited with ordinary scientific honesty.

(5) "Absolute brain capacity is measured by an X-ray photography method worked out by Stevenson and Porteus, the validity of which seems not to have been established." The brain capacity was *not* measured by photographs. As stated on page 169, it was done by taking the actual head measurements of about 4000 children. It is true that in a discussion of methods on page 139 it was incidentally remarked that if the method worked out by Stevenson and Porteus is used, error would be reduced. The reviewer is under a quite unnecessary misapprehension as to the method employed.

(6) "Between such capacity of the head and intelligence and certain desirable temperamental traits, on the other hand, a high correlation is found." A high correlation was not found. On the contrary, between intelligence and brain capacity the correlation is stated to be "probably about the order 0.3." The authors distinctly state that this correlation is not high (p. 150).

(7) "There is a suspicious enthusiasm for the method." Again and again the authors point out the pitfalls of the method and its unreliability as an individual diagnostic measure, *e.g.* "it fails to provide a reliable key to individual intelligence," (p. 138); "it is a difficult problem to state the connection of brain volume with intelligence" (p. 140). (See also pp. 147, 148, 173.)

(8) "The number of cases is, however, small and there is little satisfactory evidence that the samplings are good." The number of Japanese measured was 1307 and Chinese 1027—quite respectable totals. Further, all the evidence possible that the samplings are good was presented. "All of the children attending three large schools were measured and all the boys in a fourth" (p. 169).

(9) "We read that brain growth continues through the whole period of growth in stature even after all the 'powers' both mental and physical, have reached maturity; moreover, that whenever arrest or cessation of brain growth has occurred at an early age, idiocy or imbecility exists. This is all to be taken merely on the assertion; no evidence is weighed or presented." With regard to the evidence for the continuance of brain growth through the period of growth in stature, the reviewer's criticism is entirely mistaken. On page 153 a graph shows the brain growth curves for 1547 normals and 295 male defectives from 13 to 21 years, followed by a table on the next page. With regard to the second statement, the authors really did not consider that such a well known fact required evidence in its support. We thought that everyone was familiar with the microcephalic idiot or imbecile in whom brain growth ceased or was arrested at an early age.

(10) "Brain size is also in close relation to cultural level in the different races." Our statement reads, "A comparison of living races also shows *a* relation between average brain size and cultural level" (p. 143). Surely the reviewer sees a difference between 'a close relation' and 'a relation.'

(11) "Boys are found to be superior to girls both in brain capacity and in intellectual ability and initiative. There is a blissful disregard of most careful work done by psychologists on sex differences." This statement also requires refutation in parts. Boys were *not* found to be superior to girls in intellectual ability. Learning capacity is held to be equal if not superior in girls. We say on page 160, "Returning to the subject of sex differences, we may observe that all the evidence that has been collected with regard to sex differences appears to prove that the differentiation in male and female post-pubescent brain

growth is not related to differences in learning capacity. There is almost general agreement that females are equal if not superior to males in this respect." The authors then proceed to quote Burt and Moore with regard to female superiority or equality in memory tests and tests of higher reasoning processes. We did not make this statement attributed to us and hence we certainly would not cite authorities in its support.

(12) "For tests of intelligence, as well as of temperament, the authors . . . prefer the Porteus Maze." Here again the reviewer is mistaken. The author of the Maze test has never, in this publication or any other, preferred the test as a test of intelligence. Conversely, he has by correlation and otherwise shown that it has only a "fairly close" relation to tests of general intelligence, the correlations being "usually of the order 0.5 or 0.6" (p. 289). There is not one word in the book which is intended to indicate such a preference.

(13) The reviewer states that an historical sketch of race studies is "very inappropriately selected as to representativeness" but neglects to say that later on a whole chapter is devoted to previous studies in which the investigations by such workers as Brown, Arlitt, Garth, Kimball Young, Brigham, Colvin and Allen, Murdock, Pintner, Davenport and Craytor, Darsie, Young, Dickson and Fukuda are discussed.

(14) With regard to one criticism we agree that "J.P." is correct and that is where he states that the annotated bibliography is "remarkable for important omissions." It is unfortunately true that we neglected to list one very important work, "A Study of the Differences between White and Negro Children" by Joseph Peterson. Our manuscript left our hand in 1924 and this study, though bearing the date 1923, did not reach us until a later period and hence was not available for listing and annotation. For this omission of Joseph Peterson from the list we would crave J. P.'s indulgence.

In concluding, the reviewer passes some very severe strictures on the book especially with regard to what he calls the "marked freedom on the part of the authors from the usual limitations upon scholars of careful conformity to established facts." If the book really contained what the reviewer says it does, this criticism might be considered just, but as the case actually stands this condemnation is much more applicable to the review than the book. The latter is by no means exhaustive or complete in its treatment of the difficult question of racial psychology and many would agree that much of its data is suggestive rather than conclusive. But surely it is extremely unfair to attempt to make the authors responsible for the reviewer's misapprehensions which can only be due to a hasty or careless reading of the book.

University of Hawaii S. D. PORTEUS

A REPLY TO PORTEUS' 'PROTEST'

There was no intention of concealing who 'J. P.' is; my name occurs at the beginning of the review section, and in accordance with well established editorial practice, there is no need of repetition. The identity of the reviewer appears clear even to Mr. Porteus. I shall reply briefly to each of his "corrections" in order; but even if I were wrong in all the minor points indicated, that would not prove that my general evaluation is at fault.

(1) On page 289 the authors say: "The relation of maze test performance to so-called 'general intelligence' is fairly close." Correlations with the different intelligence tests are not very high (usually 0.5 or 0.6), we are told, but the whole treatment rightly indicates that these test scores do not register all the aspects of intelligence. There are many statements which imply that the maze test measures several important aspects of intelligence, *e.g.* "In general, therefore, the maze test provides a measure of the individual's ability to adapt his methods to the solution of a problem, the most intelligent attitude to which is one marked by forethought and prudent working" (p. 285). From there on, to page 300, this general position is taken. How shall one differentiate 'intelligence' from such mental functions? In Tables 29, 30, 31 (if I read correctly) maze scores are converted into IQs.

(2) There are numerous photgraphs in the book besides those of the boys who are listed as *typical*. These photographs involve both boys and girls, as well as adults. What are they there for, if not to typify or represent in some degree their respective races? (See pp. 32, 38, 46, 54, 64, 100). Are they not there to catch the public eye?

(3) This hardly needs a reply. But note: "among the remainder of the group" were so and so. Is this a good description of as important a factor in this experiment as were these judges? The reader of the book should note the descriptions of the several races by the authors, and then compare these descriptions for closeness of agreement with the *later* results by the judges. Does careful scientific work not require that causes of the close relationships *thus established* should be analyzed? The authors selected the judges. It is clear that several common influences operated toward the final high agreement. Were the descriptions taken from the later results of the judgments, or were they not? The reader is entitled to know.

(4) This is answered in (3).

(5) The reviewer did somewhat misunderstand the situation here, but there were some grounds for this misconception. Readers may, however, expect something more definite than general statements as to the reliability of such "brain capacity" measurements as were made, and also as to *how much* more accurate this X-ray method is than the method which was used. We should also have some quantitative comparisons with brain volume otherwise measured in post-mortem examinations.

(6) The "high correlation" to which I referred is not that given as 0.3 by the authors. My idea was gathered generally from statements like these, and an argument running through more than fourteen pages: "The study of the few human remains that have been preserved to us from historic times *would also lead to the conclusion* [italics mine] that along with the rise in culture there has been a corresponding evolution of the brain" (p. 141); "Another race which shows educational aptitudes proportionate to their average gross brain development is the African negro" (p. 145); "It is our belief that this maturing of the power of learning capacity does not by any means signify the ripening of all the mental powers and that the brain growth occurring after this age [14 or 15 years] is significant and important as subserving the development of mental traits which the psychometric yardstick is too short to measure" (p. 154). I can supply pages of quotations from the book represent-

ing the same view. In this chapter the authors admit that various pathological conditions associated with low mental level are accompanied by above-normal development of head size, thus lowering the correlation of mental level with head size, and they also explain, quite rightly again, that Pearson's low correlations between mentality and head measurement cannot be taken to indicate a low correlation with brain volume, because only single head measurements were used. All this is in line with the general position as to a high relationship, taken by the authors in this chapter on Brain Capacity and Intelligence. (The quotation attributed to me under 6 is not exactly my statement.)

(7) As to this enthusiasm the reader must judge. Pointing out a few limitations does not mean the absence of an enthusiasm. Indeed, on the strength of the authors' apparent satisfaction with the Porteus Maze test I am having it and certain other tests applied to a number of negroes, to determine how well this apparent satisfaction is justified. I, of course, agree with the authors that existing intelligence tests are unsatisfactory.

(8) The number of cases as given on pages 160 and 170 for any year and sex ranges between 24 and 78 for the Chinese children and between 27 and 100 for the Japanese. These numbers are not large. It is not a question of *totals* for both sexes and all the ages from 6 to 15 years, and that is just where the book is misleading to the general reader. The big total numbers seem even to have misled the senior author!

(9) Again numbers are not large in some of the age-groups compared in the curves on page 153. In the defectives they run from 41 to 73. Surely the authors will not try to defend the statement, that "whenever arrest or cessation of brain growth has occurred at an early age, idiocy or imbecility exists." This is only another general statement by them to support the close relationship between intelligence and brain volume or "capacity," to which I said the book holds. See (6) above.

(10) This criticism is only a quibbling over the points discussed under (6), and needs no further consideration here.

(11) Porteus here diverts the issue to matters of "learning capacity," "memory," etc. The authors' general position is that rather marked sex differences in certain important intellectual powers are well established, if I understand them. There is a confusing use of terms like 'learning capacity,' 'initiative,' 'ambition,' etc. But note this: "It is possible that we may ascribe this lesser female brain capacity in other years [than 12] to the lessened general bodily size of the girl. But unfortunately for the advocates of sex equality, from 14 years onward the boys steadily increase their superiority so that from an average of 62 cc. at 14 years of age it is increased to 128 cc. at maturity. This advantage in males is due not to any remarkable spurt in their development but simply to a retardation of brain growth in the female" (p. 158). This "lack of initiative [in women] is explained by female apologists as being entirely due to an attitude that is enforced on women by convention and public opinion" (p. 161). I here re-assert my charge of "a blissful disregard of most careful work done by psychologists on sex differences." Much more evidence for my position can be supplied if necessary.

(13) and (14) The authors give first a very sketchy and, I must repeat, "somewhat inaccurate" account of race studies. Woodworth's race tests

for example, are placed in the Chicago Exposition. In later chapters they discuss several other such tests. Since this later account also has defects of the kind mentioned in my characterization of the first one, it was not considered necessary to discuss it separately. These two accounts, and also the annotated bibliography, leave out several studies—besides my own monograph!—which were published early enough to have been considered. The authors rightly point out the inadequacies of the army testing, and yet on the basis of these army tests they say (referring to the twelve-year-old level of mentality arbitrarily set in early Binet testing as the borderline of feeblemindedness in adults) that the army examinations "proved that there were many millions of people below the Binet age who are getting along perfectly well in the community" (p. 187). It is, of course, a question of opinion whether the authors' accounts of the race testing is adequate. I accept many of their statements, but cannot agree with Porteus' apparent estimate of the general sketch.

It would be possible to go ahead and point out numerous inaccuracies and unjustifiable categorical statements in the book under consideration, in justification of my evaluation of it, but I shall not indulge in this sort of attack. If Mr. Porteus could see the marginal notes in my copy he would not charge me with hasty and careless reading of his book! Let it not be inferred from these replies to his 'corrections' that I see no merits in the work of these authors. They have done an extensive piece of pioneering work, which is sure to stimulate further research. It is only by continued investigations that we reach in time something that is thoroughly satisfactory and reliable.

George Peabody College JOSEPH PETERSON

THE THIRTY-SEVENTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The thirty-seventh annual meeting of the American Psychological Association was held at Columbia University December 17–29, 1928. A registration of 537 members and associates makes this the largest meeting in the history of the Association.

At the business meeting 16 new members and 206 new associates were elected. The election committee reported the election of Dr. K. S. Lashley, of the Chicago Institute for Juvenile Research, as President for the year 1929. It was decided to merge the 1929 meeting of the Association with the meeting of the Ninth International Congress which will be held at Yale University September 1 to 7. Provision will be made for the address of the President as a part of the program of the Congress. The Council of Directors of the Association was given power to deal with all matters of business usually handled at the annual business meeting of the Association. The rapidly increasing size of the association has made the annual banquet such a serious problem that with the unanimous support of the members it was voted that the banquet be discontinued hereafter. This interesting social feature of the meetings will doubtless be replaced by dinners and smokers for smaller groups.

Compared with earlier meetings of the Association there were relatively fewer formal papers and more round tables for discussion of special problems. Certain interesting shifts in emphasis were perceptible in the program as a whole. For instance, there were two sessions devoted to animal psychology at which ten papers were read, while for mental tests there was one session with five papers. One session was devoted to the Psychology of the Emotions, and of the five papers read three dealt with the galvanic reflex and a fourth was concerned with other bodily expressions of the emotions. There were only four papers read at the session for Educational Psychology and at least one of these might have been included within the Experimental group. There were no sessions devoted to Applied Psychology or Industrial Psychology, and no papers included in other sessions that would seem to come under this head. As the program was necessarily constructed out of the material offered by the members of the Association, the papers presented give an interesting index of the trends of research. One session was devoted to General Psychology with five papers, two to Experimental with twelve papers, one to Child Psychology with five papers and one to Abnormal with five papers.

Two sessions were arranged for nineteen Informal Reports by Graduate Students. These reports were of uniformly high quality and comprised a valuable part of the program. They were drawn from twelve different Universities, and covered a very wide range of research interests.

Six Round Table discussions were scheduled, each with a leader and a designated group of persons ranging in number from 25 to 60 who had indicated an interest in the topic. The topics assigned were: Clinical Psychology, Personality, The First Course in Psychology, Consciousness and Behavior, Psychophysical Measurement, and Esthetics. These meetings were, on the whole, disappointing. The groups, more than a hundred in every case, were too large to preserve the atmosphere of a round table, and the contributions in many instances were not constructive. For example, in the discussion of Consciousness and Behavior there was little or no progress beyond the search for suitable definitions of such terms as Behavior, Adjustment, Situation, Consciousness, etc.; and in the case of the First Course in Psychology the contributions were limited mainly to an account of how different individuals teach the first course, with no progress in the direction of a 'one best way,' if such there can ever be. The experiences of a seminar group, which worked under the direction of R. M. Elliott and spent six weeks upon an intensive study of the First Course problem (a resumé of which was presented at the Round Table) suggests that Round Tables will have to give way to Work Tables, consisting of a number of small, carefully chosen groups to study very specific aspects of a problem.

One of the most profitable discussions occurred in the Round Table on Psychophysics where attention was directed to such specific problems as the use to be made of 'equal' judgments in the psychophysical methods and the relative significance of the threshold and of some measure of variability such as the standard deviation of the judgments.

The conference of experimental psychologists, under the chairmanship of R. S. Woodworth, held two sessions. The meetings were informal. After a census of the topics upon which members were prepared to report, sensation was chosen as the first topic. In this field experimental studies of audition were most prominent. Among later topics the nature of the learning curve received some attention. From the point of view of the mathematics of the curve and from experimental data, it was suggested that the present form of the learning curve is unsatisfactory.

The annual banquet held at the Hotel Pennsylvania was attended by 238 persons. About 150 others came to hear the address of President E. G. Boring which followed the dinner. James McK. Cattell, who reported concerning the Ninth International Congress, pronounced the group the largest gathering of psychologists in history. Dr. Boring's address was entitled "The Psychology of Controversy." By citing instances of controversy among great psychologists past and present he showed that controversy, even of the highly personal sort, may be an instrument of progress, and is an almost necessary accompaniment of the clash of brilliant minds and keen interests in research.

Columbia University A. T. POFFENBERGER

UNLEARNED BEHAVIOR OF THE ALBINO RAT

The following observation of an unlearned reaction in the albino rat was made several summers ago. The author was visiting a commercial rat colony in Philadelphia. Rats were sold with ages known to within three days. The adult females had their litters in individual cages and kept them there until weaned. Then all the young rats born within the three day period were placed together in a large cage. These cages had wooden sides about 2 ft. high, a wooden floor about 6 ft. by 3, and a half-inch mesh wire screen over the top. The visit was made on a hot, humid day. From 250 to 400 rats were kept in such a cage.

The author was surprised to find a great many rats hanging from the wire mesh apparently dead. The first interpretation was that the rats had jumped to the screen for air or to escape and had caught their noses in the wire. Actually, these rats were sleeping and maintained the position by hanging by their upper teeth from a wire of the screen. If awakened by scratching the nose, they would throw back the head disengage the teeth from the wire and drop into the milling mass below. So far as I can discover, no mention of this form of behavior occurs in the literature. The observation was reported to Professor Henry H. Donaldson of the Wistar Institute of Anatomy and to a number of other individuals who have had a wide experience with rats, but no further information was obtainable.

This reaction was probably unlearned, although there is the remote possibility that each group of rats worked out an identical reaction as the appropriate one to this new set of conditions. It will be remembered that the rats in a given cage were all born within a three day period; that they were never placed with older rats (except the mother) and were then in smaller cages; and that they were observed sleeping in this hanging position from all of the large cages. The reaction is obviously purposeful: it brings the rats out of the press on the floor during sleep and allows the cooler air to get to all sides of the body.

This observation is reported primarily to get it into the records but also as a beautiful example of the fact that one may have the appropriate neural mechanism for unlearned behavior (the author almost used the word instinct) totally unsuspected until the appropriate stimulus comes along to set off the reaction. The type of cage which has been developed for handling rats in the experimental laboratories, with the wire on the front, would not present the conditions for proper stimulation; lack of crowding removes another condition.

The author suspects the essential conditions are: (1) a cage of such form that the reaction is physically possible; (2) overcrowding; and (3) heat and possibly humidity. He has never had the chance to make observations on another group of rats to analyze the conditions. This note is published in the hope that further observations may be made.

University of Pennsylvania SAMUEL W. FERNBERGER

A BIBLIOGRAPHY OF THE ATTEMPTS TO IDENTIFY THE FUNCTIONAL END-ORGANS OF COLD AND WARMTH

During the years since the discovery of the temperature spots[1] a number of investigators have attempted to identify the functional end-organs. Two methods have in general been used; a direct method in which the tissues under a cold or warm spot have been excised and examined histologically and an indirect method in which correlations have been drawn between the spread and distribution of the cutaneous end-organs and the prominence or absence of the specific cutaneous qualities. The direct experimental method, the first employed, has been used five times. It was used by Donaldson in 1885,[2] by Goldscheider in 1886,[3] by Haggqvist in 1913,[4] by Dallenbach in 1917,[5] and by Pendleton in 1928.[6] It has thus far led to negative results; nerves and undifferentiated endings but no specialized end-organs have been discovered in the excised tissues. The indirect method, used first by von Frey[7] and recently by Strughold,[8] and Strughold and Karbe, has led to positive correlations which though tentatively advanced, have been dogmatically accepted.

The purpose of this note is to call attention to Goldscheider's study[3] by the direct method. For some inexplicable reason this has been overlooked; it is not mentioned in any of the investigations which follow it. It is an excellent study, done with the thoroughness that characterizes all Goldscheider's work, and deserving of a prominent position in the literature on this problem.

K. M. D.

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[1] Magnus Blix, Experimentelle Beiträge zur Lösung der Frage über die specifische Energie der Hautnerven, *Upsala läkarefören. fårhandl.*, 18, 1883, 87 ff., also in *Zsch. f. Biol.*, 20, 1884, 141-156. Alfred Goldscheider, Die specifische Energie der Temperaturnerven, *Monatshft. f. prac. Derm.*, 3, 1884, (cf. *Ges. Abh.*, 1898 Bd. I, 53-76). H. H. Donaldson, On the temperature sense, *Mind*, 10, 1885, 399-416.

[2] Donaldson, *op. cit.*, 408 f.

[3] Goldscheider, Histologische Untersuchungen über die Endigungsweise der Hautsinnesnerves beim Menschen, *Arch. f. Anat. u. Physiol., Physiol. Abtheil., Suppl.-Bd.*, 1886, 191-227.

[4] Gösta Häggqvist, Histophysiologische Studien über die Temperatursinne der Haut des Menschen, *Anat. Anz.*, 45, 1913, 57 ff.

[5] K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 424 ff.

[6] C. R. Pendleton, The cold receptor, *ibid.*, 40, 1928, 353 ff.

[7] M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. königç. sächs Gesell. d. Wiss., math.-phys. Kl.*, 47, 1895, 181-184.

[8] H. Strughold, Die Schwellen des Kältesinnes am Auge, *Zsch. f. Biol.*, 83, 1925, 201-206; Die Topographie des Kältesinnes in der Mundhöhle, *ibid.*, 515-534. H. Strughold and M. Karge, Die Topographie des Kaltesinnes auf Cornea und Conjunctiva, *ibid.*, 83, 1925, 189-200; Die Dichte der Kaltpunkte im Lidspaltenbereiche des Auges, *ibid.*, 207-212; Vitale Färbung des Auges und experimentelle Untersuchung der gefärbten Nervenelemente, *ibid.*, 297-308.

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THE EFFECTS OF DIRECT STIMULATION OF THE BLIND-SPOT

By Harry Helson, Bryn Mawr College

The blind-spot has engaged the attention of investigators in almost every field of science, furnishing a common ground for the physicist, biologist, physiologist and psychologist. To the first it still offers problems in connection with the course of light through the media of the eye. For the biologist, numerous questions concerning finer structure and development remain to be answered. The physiologist has found it of importance in the functional problems it raises with respect to the part played by the optic papilla in the field of vision; to him, as we shall see, it has too often been a source of hopeless despair, so far as arriving at a satisfactory explanation of the phenomena observed is concerned. The psychologist has made much of it in his treatment of perception. It is impossible to separate these various aspects of the numerous problems comprehended under the term 'blind-spot,' for when it is found that this area is not always blind and does not function as a blind spot properly should, then it is necessary to consider the facts from the ground up.

The blind-spot has been involved in nearly every controversy regarding the eye and its functions since its discovery 260 years ago. It has played a prominent part in the discussions concerning the seat of vision in the eye, the ability of the optic nerve fibers

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*Accepted for publication December 28, 1928.

to respond to light and electrical stimulation, facts and theories of binocular and monocular space perception, the locus of contrast and other observed visual phenomena—in short, in every moot question concerning vision.[1]

Since the relation between light and electricity, the electrical nature of the nerve impulse, and the effect of light on protoplasm have been under investigation, the problem of vision in the blind-spot assumes importance once more if it is true that the optic nerve fibers respond to direct stimulation by light or electricity. If we believe, as many biologists at the present time are inclined to believe, that all protoplasm responds more or less to light, and that the action of light on the optic nerve fibers is but a special case of the close relation existing between electrical or electromagnetic and light phenomena, then stimulation of the optic nerve fibers by light to see if excitation, conduction and sensation are possible seems to offer a promising field of investigation. If the optic nerve fibers can be shown to respond to direct stimulation by light, we shall be able to envisage the mechanisms of vision as well as the broader problems of nerve conduction and specificity of neural energies in a new manner. Nor can our conclusions be devoid of practical implications ultimately because of the rôle played by the blind-spot in tests of scotoma, diagnosis of disease and a host of other ways.

The present study is mainly qualitative, for reasons to be mentioned later, but we hope to follow it soon with quantitative measurements which should throw light on some of the problems our present treatment does not leave wholly satisfactory. The writer realizes the opportunity and necessity for quantitative work in a problem of this kind. But careful descriptive work seemed necessary in order to determine *what* happened under conditions of direct stimulation, and having done this, we decided to leave the other for a later paper. Nor can the psychologist be expected to handle his side of the problem adequately until he gets more help from the physicist on the behavior of light in non-homogeneous systems like the eye, from the anatomist on the finer structure of the optic nerve head, and from the physiologist on the effects of direct stimulation of nerve tissue.[2]

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[1] Textbook accounts of the phenomena in the blind-spot hardly do justice to what is found there nor do they indicate what a large literature has been devoted to the subject. Helmholtz and Tschermak (*opp. citt. infra*) have given excellent accounts.

We shall begin with an historical and critical survey of the work reported in the literature on the blind-spot, proceed to an account of our own experiments and what they seem to mean, and conclude with a discussion of the theoretical aspects of the subject.

HISTORICAL AND CRITICAL SURVEY

Immediately after Mariotte announced his discovery of the blind-spot in 1668[3] a controversy arose as to the following facts: (1) the actual size of the blind-spot; and (2) the seat of vision in the eye. Mariotte believed the organ of vision to be the choroid because the retina, or rather the nerve layer of the retina, covered the optic nerve whereas the choroid ceased there—hence since there is no vision in the optic disk it is because the choroid is lacking in that region. Not only did Pecquet deny that the whole area corresponding to the optic disk was blind but he asserted that the proper organ of vision was the retina. Supporting Pecquet were Volkmann and Coccius who limited the blind-spot to the region of the retinal artery and vein. Volkmann[4] was quite explicit in his belief for he says, "Dieser von Mariotte zuerst ausgeführte Versuch gab eine Zeit lang zu dem Irrthume Anlass, dass der Sehnerve nicht als Leiter des Lichtreizes fungire." Referring to Rudolphi's hypothesis that only the artery is insensitive to direct stimulation by light, Volkmann continues "und diese Hypothese ist durch Versuche von mir bestätigt worden." But he does not tell us what his experiments were which prove conclusively that the optic disk responds to direct stimulation; he merely states that his method of calculating the size of the blind-spot cannot be subject to error.

Later work has shown that under ordinary conditions of mapping the blind-spot does coincide with the whole area of the optic nerve as it enters the eyeball and to this extent Mariotte was right. But he was wrong if the conditions are slightly changed for we now know that the size of the blind-spot depends upon the conditions under which it is determined. The factors and Brückner[12] definitely reject the possibility of physiological irradiation in the retina along with Helmholtz.

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[2] For the structure of the optic nerve head one may consult works on anatomy which are not very satisfactory. An extended monograph with excellent plates will be found by A. Elschnig, Der normale Sehnerveneintritt des menschlichen Auges: Klinische und anatomische Untersuchungen, *Denkschriften d. k. Akad. d. Wiss. zu Wien*, math. naturwiss. Klasse, 70, 1901, 219-303. It should be remembered that the optic nerve fibers lose their medullation at the *lamina cribrosa*, coming through the optic nerve head as naked axones, resembling in this respect the neurones of the central nervous system. H. S. Gradle, The blind spot, *J. Amer. Med. Ass.*, 77, 1921, 1483, has found abnormal cases of medullation of fibers *but never in the optic disk.*

[3] *Oeuvres de Mariotte*, published by Jean Neaulme, 1790, II, 496-534. The controversy between Pecquet and Perrault on the one hand, against Mariotte on the other, is given here in a series of letters that passed between them. I have been unable to find the reference given by Helmholtz and others to Mariotte's announcement in *Phil. Trans.*, 1668.

[4] A. W. Volkmann, article on *Sehen*, in Wagner's *Handwörterbuch der Physiologie*, 1846, III, 265-351.

Before leaving this topic we must refer to a number of facts in the literature which cannot be dismissed by vague reference to ready-made terms and theories. While some of these facts are by no means as clear-cut as we should like to have them, they take on new significance in the light of experimental work designed to test the traditional theories. Polimanti[13] cites Baumgarten as having extirpated the eye-ball, leaving a healthy optic nerve which, when stimulated with light, gave rise to a sensation of light. Here, of course, the cut ends of the nerve were stimulated, making conditions different from ours where axis cylinders are being stimulated along their trunks. Brewster[14] reported a case in which he believed the retina was not functional while vision was still preserved to some extent. But his account is not satisfactory as regards the type of injury to the retina and the amount of vision preserved. Most important of all evidence on this point to date is that given by Stern and Feinberg.[15] Stern found that observers saw *phi* movement from stationary stimuli exposed in the blind-spot when neither stimulus alone could be seen. This finding shows that the amount of light that could reach adjoining parts of the retina is negligible and is in complete agreement with the well-known fact that the limen for movement in normal vision is lower than the limen for color. In this case, the theories assuming irradiation and the like, are hard put to it to explain how a subliminal stimulus within the blind-spot finds its way outside, gives rise to apparent movement which is then seen, not between the places stimulated on the sensitive retina, as in all other cases of seen movement, but is localized in the original place of stimulation within the blind-spot.[16]

Feinberg, whose results we have substantiated in every respect and shall have occasion to refer to later, found that the haze or halo which usually surrounds the core of light seen in the blind-spot when it is stimulated directly is smaller than the projection of the blind-spot—a fact which our observers have repeatedly reported. This author points out that we should have to assume in this case that a subliminal stimulus in the portions adjoining the blind-spot can evoke a supraliminal effect within the blind-spot. He also points out that there is no sudden increase of brightness at the boundaries of the blind-spot nor does the bright core or halo take on the form of the blind-spot as it should if the theories of outside stimulation are correct.

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[12] A. Brückner, Ueber die Sichtbarkeit des blinden Fleckes, *Arch. f. d. ges. Physiol.*, 136, 1913, 610–657; Zur Lokalisation einiger Vorgänge in der Sehsinnsubstanz, *ibid.*, 142, 1911, 241-254.

[13] Cited by O. Polimanti, *op. cit.*, who gives as reference only Nagel's *Handbuch der Physiol.*, 1905, vol. 3. Since no page reference was given I have been unable to find it in the third volume of Nagel. The name of Baumgarten does not help either, hence I give this statement on the authority of Polimanti.

[14] D. Brewster, Account of a case of vision without retina, *Report of the British Assoc. at Belfast*, 1852, 3. He suggests that vision in this case was mediated by the choroid, going back to Mariotte's view.

[15] A. Stern, Die Wahrnehmung von Bewegungen in der Gegend des blinden Flecks, *Psychol. Forsch.*, 7, 1926, 1-15; N. Feinberg, Experimentelle Untersuchungen über die Wahrnehmung im Gebiet des blinden Flecks, *ibid.*, 7, 1926, 16–43.

[16] A. Basler, Über das Sehen von Bewegungen, II. Die Wahrnehmung kleinster Bewegungen bei Ausschluss aller Vergleichsgegenstände, *Arch. f. d. ges. Physiol.*, 124, 1908, 332 ff. This author denies that the movement seen in the blind-spot when both stimuli are falling in it is due to direct stimulation of the optic papilla. He believes that surrounding retinal elements are responsible.

The evidence from electrical stimulation of the blind-spot would be more conclusive if we were sure that only the optic nerve were being stimulated when the current is passed through the head. Since the optic disk appears against the rest of the field in a complementary color it seems as if we are dealing only with a contrast effect, and indeed some writers have explained the facts in this way. Whether or not this explanation will ultimately hold we cannot say until more work has been done along this line.

The difficult, and probably most crucial, question to be answered if we grant that the optic nerves respond to direct stimulation is that concerning eccentric projection, a difficulty which we meet with in the theories which deny that there is vision in the optic disk. If the optic nerves are responding to stimulation, why is not the whole retinal field illuminated or the places served by the stimulated nerves, as we should expect when the trunk of a nerve responds to stimulation? Since the same objection applies to the theories which assert that elements outside the optic disk are being stimulated, an objection which Helmholtz believed to be fatal to them, we must leave it *dahingestellt* until far more is known concerning the facts of nerve conduction and mechanisms of localization.

Vision in the blind-spot. While most writers deny that we see *with* our optic disks some are ready to admit that we see certain phenomena *where* our optic disks are. In other words it seems to be fairly well established that under proper conditions the following types of phenomena may be observed in the region of the blind-spot: after-images, contrast effects, distortions within regular lines, the optic disks themselves, and certain effects due to electrical stimulation of the optic nerve. On the other hand, the failure to obtain evidence of the presence of the blind-spot in the visual field under certain conditions has led many writers, including some very great names, to deny that anything at all is seen either with or in the blind-spot.[17] The result has been that every fact regarding either vision in the blind-spot or absence of vision in the blind-spot has been subjected to criticism and prolonged controversy, each surface is clearly a case of contrast. If one looks at a dark background the disk should appear white or light gray according to the law of contrast, but Brückner found that it looked a deeper black than the background until the general illumination of the room was decreased. Then the blind-spot appeared in 20–30 sec. first as a deep, dark disk with bright halo which gradually lightened until it disappeared in the background. Brückner then fixated a field half black, half white so that the blind-spot fell upon the boundary of the two fields, and came out with the following generalization verified by Tschermak: "If the blind-spot falls on a half black, half white field, it will take on the brightness of the field that falls on the temporal side of the blind-spot, the middle being dark if the completed field is white, white if the completed field is dark." This law holds for colored fields like red-green and yellow-blue even though observation with colored fields is much more difficult.[23] Due to local adaptation, eye-movements, and the faint appearance of the disk, as we ourselves have found it, it is very difficult 'to catch' the phenomena Brückner describes although there can be no doubt of the facts he states.

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[17] Positive statements to the effect that we never see in the blind-spot when placed against equally positive ones that we do see in the blind-spot would be amusing were it not for the state of affairs they disclose among supposedly careful scientific workers who, in presenting their conclusions, fail to consider the facts opposed to their theories and will not take the trouble to investigate them under the conditions which gave them birth. I give a few representative statements from leading writers on the blind spot: "Sie ist völlig blind"—Fröbes; "la papille optique aveugle au sens propre du mot"—Charpentier; "the blind-spot is filled in by the mind"—Volkmann, Bunge, Fick, and Du Bois-Reymond; "Sie ist ein psychologisches Nichts"—Werner; "Il serait absolument inutile de vouloir démontrer aujourd'hui que la papille du nerf optique est insensible à la lumière. Ce fut déjà démontré des l'epoque de Mariotte"—Polimanti. On the other side we have such positive statements as this from Tschermak, who, however, does not believe the optic disk itself is functional, "Under special conditions it is possible to get an indubitable, noticeable sensory impression from the blind-spot." The positive reports of vision in the blind-spot have received little attention in theory where what is seen is explained away on the assumption that the optic disk "au sens propre du mot" is blind.

Contrast effects have been obtained in two more ways very different from those mentioned: (1) In pathological color scotoma, reported by Wundt,[24] and (2) from electrical stimulation of the optic nerve. Although there seems to be no reason to doubt the validity of Wundt's observations on his own scotoma, nevertheless Landolt[25] denies that there is any filling in either of the blind-spot or of acquired scotomata. So far as electrical and electro-magnetic stimulation of the optic nerve are concerned no general rule can be laid down regarding the effect of direct and reversed currents and of make and break. Tschermak[26] in summarizing the work of 31 authors who have worked with this form of stimulation generalizes as follows: "stimulation of the eye with constant current (which is now regarded as descending the optic nerve) results in a darkened visual field somewhat colored, except for the blind-spot which appears bright and in essentially complementary hue; if the current is continued sufficiently long and is sufficiently intense, when it is broken the bright optic disk gives place to a dark disk while the rest of the field becomes considerably lighter. The last effect may be regarded as the negative after-image of the first. Making a current which is regarded as ascending the optic nerve brightens the general field and darkens the blind-spot while breaking the current gives rise to the opposite effect." We thus find typical contrast effects at make and break and in reversal of current.

Magnusson and Stevens[27] found light sensations in the region of the blind-spot with changes in strength of a magnetic field around the head, but they point out it is impossible to say whether the sensory elements of the retina or the optic nerve fibers alone are responsible for what is seen.

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[23] Brückner, *ibid.*, 628.

[24] W. Wundt, *Grundzüge der physiologischen Psychologie*, 1910, II, 539.

[25] M. Landolt, Beobachtungen über die Wahrnehmbarkeit des blinden Fleckes, *Archiv. f. Augenhk.*, 55, 1906, 108-118.

[26] A. Tschermak, Ueber Merklichkeit und Unmerklichkeit des blinden Fleckes, *Erg. d. Physiol.*, 24, 1925, 330-378.

[27] C. E. Magnusson and N. C. Stevens, Visual sensations caused by changes in the strength of a magnetic field, *Amer. J. Physiol.*, 29, 1911, 124-136.

The final upshot of the literature on the contrast effects in the blind-spot seems to be that except in cases where it is definitely known that the optic fibers are not being directly stimulated more than surrounding retinal elements, *e.g.* when viewing a uniformly illuminated surface on which the optic disk is seen, we cannot agree with those who maintain that all the phenomena observed in the blind-spot are contrast effects arising from the positive action of adjacent retinal elements while the optic disk plays only a negative rôle in the sense of a colored shadow. While it is true that we cannot be sure what places are being stimulated when vigorous eye-movements are made and when the electric current is applied to the eye, the contrast explanations advanced especially by Brückner and Tschermak offer no solution to the facts of direct stimulation of the optic disk when we know that adjacent regions of the retina cannot be reponsible for what goes on in the blind-spot. For those cases where the optic disk is not being directly excited contrast theories may apply; in other cases they do not do justice to the positive part played by the optic disk in vision, albeit under restricted conditions.

Filling in, distortion, and spatial values of the blind-spot. Perhaps the greatest disagreements in a subject filled with conflicting opinions exist with regard to the filling in, distortion, and general spatial character of the field in the region of the blind-spot. Positive assertions that the blind-spot is filled in and that spatial values in its vicinity are not distorted are met by equally positive ones that corresponding to the blind-spot is a gap in the field of vision and that straight lines suffer distortion in the areas contiguous to the blind-spot. Nor can we be sure of any writer's final opinion after we have put together all he has said on the subject, especially when he is not consistent. In this welter of opinion Aubert's modest statement that he is not sure of what happens in the region of the blind-spot after hundreds of observations makes us feel that he has bowed to the facts and is reserving judgment, while Werner's dogmatic statement that there is never any break in continuity in the place occupied by the blind-spot and that "unsere selbstverständliche Voraussetzung ist, dass wir mit dem blinden Fleck absolut nichts sehen" impresses us with its neglect of well-attested facts in the literature in an obvious attempt to defend his own theory. Our experience has been that observers report different things under the same conditions, change their reports with practise, and are subject to influences outside the experimenter's control and hence it is most dangerous to generalize from one's own experience or from one set of conditions to all. It is nowhere more true than here that what one gets is a function of his conditions and nowhere have writers overlooked this simple rule of induction more than in the work on the blind-spot.

The weight of opinion is against any shortening or deformation of lines or surfaces when the blind-spot falls upon either a straight line or a homogeneously colored surface. Among those holding this view are E. H. Weber, Volkmann, Fick and Du Bois-Reymond, Aubert, Helmholtz and his student Woinow, Landolt and Brückner.[28] But Volkmann (who is not consistent), tion of why the visual field shows no deformation under certain conditions when we should expect it to. Werner assumes that since nothing is seen in the blind-spot—*sie ist ein psychologisches Nichts*—all points that fall upon it fall upon a single point and hence shrink to that point making, *e.g.* parallel lines falling there appear like two Vs base to base. But the very fact from which Werner starts to base his theory we must deny, since, where he insists *eine Fläche gleichmässig weiss* will be seen (fixate a white sheet of paper and the projection of the blind-spot is supposed to be filled in) the writer sees very plainly a purple disk in ordinary room illumination, making a break in the continuity of the white field. Indeed, with practice, anyone may verify this for himself.

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[28] E. H. Weber, *Ber. d. k. Sächs. Ges. d. Wiss.*, 4, 1852, 185; Volkmann *ibid.*, 5, 1853, 27; Fick and Du Bois-Reymond, Aubert, Helmholtz, Landolt and Brückner, *opp. citt.*; M. Woinow, Ueber das Sehen mit dem blinden F lec und seiner Umgebung, *Arch. f. Ophth.*, 15, 1869 (2 Abt.), 155-166.

Closely related to the nothing theories are those theories which ascribe a negative or privative character to the blind-spot. Among those who hold to this type of theory are Weber, Ferree and Rand, and Lohmann.[34] As stated by Weber, who seems to believe that we never experience the blind-spot either as a dark or light colored disk within uniform grounds, we perceive no deformation in the visual field because in order to have any sense of intermediate spaces we must be sensitive to places between those actually stimulated. Since, Weber goes on to say, nothing is seen between the edges of the blind-spot we have no sensation of anything between, and hence nothing is lacking and no hole appears in the field of vision. But later on Weber, who by no means holds to his theory without modifying it as he goes along, tells us that we do not see the blind-spot because it is surrounded by sensitive parts of the retina and "so viel ist gewiss, dass wir den Zusammenhang der Dinge, die in die nicht sichtbare Region des Sehfelds hineinreichen, so sehen, wie sie am einfachsten und am wahrscheinlichsten ist." He thus finally resorts to a probability theory, closely allied to the psychic theories discussed later. He even goes so far as to deny that we perceive the blind-spot as dark when the rest of the field is light in following out the implications of his first theory. Here theory must bow to fact for the evidence since Weber's time is overwhelmingly in its favor.

All forms of the vacuum theory break down because of the wide range of positive phenomena observed in the blind-spot under various conditions. While the vacuum theories may hold for casual observation and under conditions which do not permit the optic disks to appear as differentiations within the visual field, the careful work of those who have found vision in the blind spot cannot be ignored.

(2) *Psychic theories.* These theories in their various forms aim to explain how the gap which should be seen in the monocular field of vision owing to the presence of the blind-spot is bridged. Among the writers who have expressed themselves in favor of these theories are Aubert, Helmholtz, Fick and Du Bois-Reymond, Bunge, Volkmann, H. Müller, E. H. Weber, and Wundt. The filling in may be due to imagination, judgment, association, or the mind acting in accordance with the laws of probability.

In conformance with the probability form of the psychic theories Weber and Volkmann assert that if two qualitatively unlike things border on the blind-spot we see them connected through the blind-spot in the simplest and most probable manner. Volkmann also believes that imagination can help quite a bit in difficult cases,[35] *e.g.* when looking at a page of print we see the letters filled in where the blind-spot falls. Here it is *das geistige Auge* which sees them. Aubert[36] implies that the burden of proof lies on those who expect not to see fields filled out for he says that we see a field as continuous *donec probetur contrarium.* A slightly different and more acceptable theory was offered by Fick and Du Bois-Reymond[37] which means something very different from the other probability theories: having noted that one of several phenomena may be seen in the blind-spot under given conditions they come to the conclusion that what happens is a matter of chance, but neither will nor imagination is responsible.

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[34] E. H. Weber, *Ber. d. k. Sächs. Ges. d. Wiss.*, 4, 1852, 185; Ferree and Rand, *Amer. J. Physiol.*, 29, 1912, 398-417; Lohmann, *Arch. f. Augenhk.*, 81, 1916, 181-196.

Extreme forms of the imaginative and associative theories have been held by Heine, Volkmann, and Bunge. For Volkmann sensation and imagination play together to give us our perceptions; imagination is responsible for the unexplainable phenomena seen in the blind-spot. Bunge would even take the whole problem out of the domain of science: "Diese Ausfüllung der Lücke mit der Farbe der Umgebung ist offenbar ein psychische Process und lässt sich physikalisch nicht erklären."[38]

The filling in of the blind-spot assumes practical and clinical importance in the testing of visual scotoma. Heine[39] maintains that patients with scotoma may complete dot figures just as is done in the blind-spot. Hess[40] found no filling in of the blind spot with his dot figures and Landolt[41] supports him against Heine whose observers do fill in the dots! On the other hand, we have Wundt's word for the fact that in the case of his own scotoma filling did take place. Wundt also asserts that the blind-spot has the same spatial values as the rest of the visual field because it is filled in with the dominant light impressions (*vorherrschenden Lichteindruck*). Wundt's theory is diametrically opposed to the theory advanced by Ferree and Rand to explain the same set of facts and there seems to be no hope of reconciling the opposed views of Hess and Heine.

A final form of psychophysical associative or probability theory has been effectively disproved by Feinberg and in the present paper, we believe. It assumes that when the blind-spot is directly stimulated with light the reason for seeing the central bright core with halo is because the greatest amount of light will fall on the contiguous retinal elements as a result of scattering. The blind-spot is then filled in as a natural continuation of this light gradient established on the boundaries of the blind-spot. But when a ring of light is exposed so as to fall partly within and partly outside the blind-spot, thus establishing the gradient, the central bright core is absent even if some filling does take place. The expected completion does not occur.

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[35] Volkmann, *Ber. d. k. Sächs. Ges. d. Wiss.*, 5, 1853, 27.

[36] H. Aubert, *Physiologie der Netzhaut*, 1865, 257 ff.

[37] Fick and Du Bois-Reymond, *op. cit.*, *Müller's Arch. f. Anat. u. Physiol.*, 1853, 396-407.

[38] G. v. Bunge, *Lehrbuch der Physiologie des Menschen*, 1905, I, 113.

[39] L. Heine, Kritik der Hesschen "Bemerkungen" betreffend den Nachweis zentraler Skotome, *Arch. f. Augenhk.*, 52, 1905, 236.

[40] C. Hess, Neue Beobachtungen an total Farbenblinden, *Heidelberger Berichte*, 31, 1903, 290.

[41] E. Landolt, *op. cit.*, *cf.* note 25.

(3) *Physiological theories.* Although it is impossible to draw any sharp line between physiological and physical theories, we shall do so for the purpose of simplifying the discussion.

(*a*) *Fatigue theory of J. Müller.* According to this theory the blind-spot merely appears blind because it fatigues so readily. The decrease in size of the blind-spot after it is first seen, the quick disappearance of the visible disks on homogeneous backgrounds and the rapid adaptation to weak stimuli noted below attest the correctness of this theory in so far as it recognizes the basic facts of lowered sensitivity and quick adaptation in the blind-spot. While it may be incorrect to say that fatigue is responsible for lack of vision in the blind-spot under all conditions, in so far as fatigue and adaptation and lowered sensitivity are related this theory summarizes important truths.

(*b*) *Theory of irradiation.* The concept of irradiation has been used in various senses by various writers. It is difficult to determine always whether true physiologic irradiation is meant or some form of scattering of light in the media of the eye which is a physical theory. Helmholtz[43] has pointed out the difference between what we shall call physical irradiation and physiologic irradiation when he distinguishes the following two types: (1) the first type may be described as arising from the fact that highly illuminated areas may appear larger in the visual field than they are in the physical field, while the adjoining dark areas appear correspondingly smaller than they are physically. This effect can be best observed when the eye is not exactly accommodated for an object although it is always present even in the best accommodated eye due to the imperfections of the refracting media of the eye. Three effects of irradiation may be observed: (a) adjacent bright areas tend to flow over into each other; (b) bright areas appear magnified; and (c) straight lines may be deformed. Irradiation is greatest when the blur circles on the retina are greatest, *hence to prevent irradiation factors responsible for the formation of blur circles must be eliminated.* But it must not be taken for granted that awareness of blur circles accompanies irradiation phenomena. To account for the fact that we see lighted areas in sharp outline when the image on the retina is not clear-cut as a result of irradiation the hypotheses of simultaneous contrast in their psychological and physiological forms have been proposed. But irradiation has also been used to explain sharp images and hence has been rejected as a plausible explanation. The contrast theories assume that contrast produces a brighter edge to the image and darker edge to neighboring regions thus making sharp boundaries (Hering, Fröbes, and Tschermak).

But a really physiologic theory of irradiation such as has been advanced by Plateau, Brewster, J. Müller, Exner, and others, and denied by E. H. Weber, Helmholtz, Aubert, Mach, Hering, and Tschermak regards the irradiation effect as due primarily not to the scattering of light in the media of the eye but to transmission or diffusion of excitation from one place (retinal element if a peripheral theory, ganglion cell if a central theory) to adjoining regions which themselves are not directly stimulable by light. Following these assumptions, Plateau[43] explained the filling in of the blind-spot by assuming that when a retinal fiber is stimulated it induces a state of excitement in neighboring fibers so that the latter arouse sensations of light without being acted upon by light themselves—a form of synaesthesis. Helmholtz rejected this theory for, if it were true, he says, it would be impossible to localize or receive single sensations, fusion would take place between the sensations aroused by the primary stimulation and the secondarily aroused sensations. So Helmholtz accepts the irradiation hypothesis only in so far as it means the formation of blur circles on the retina. We shall have to ask if the blur circles formed in the eye can account for what is seen in the blind-spot.

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[43] Helmholtz, *op. cit.*, II, 186-203.

E. H. Weber[44] was one of the first to point out the contradictions arising from any kind of physiologic irradiation theory. He says that we cannot speak of irradiation when no light is present and the blind-spot is filled in, *e.g.* when a black field appears homogeneous through the region of the blind-spot; moreover, there can be no irradiation of excitation through a region devoid of receptors like the optic disk. Tschermak also rejects the irradiation theories because of their inner contradictions. He shows that in some instances irradiation accounts for facts covered by scattering or aberration of light in the eye thus accounting for the imperfections of our optical mechanism, while in others it is invoked as a compensatory process for these very defects. Both Helmholtz and Tschermak find the physiologic theories of irradiation unnecessary and useless since well-established principles of physics and physiological optics will account for the stimulation of retinal elements around the optic disk. But they do not tell us what these principles are! Certainly the blur circles of Helmholtz will not extend sufficiently far beyond the central, maximum place of stimulation to account for what is seen in the blind-spot when it is stimulated directly by light. Nor will blur circles furnish sufficient intensity to adjoining retinal elements to account for the bright central core when the optic disk is directly stimulated. It would be necessary for the blur circles or diffraction bands from a 2 mm. stimulus at 4 meters distance from the observer to be effective over a distance several hundred times the width of the first maximum band corresponding to the region of the disk where the light from the stimulus is actually falling! Whereas we know that with a stimulus of medium intensity at this distance probably not even the second band beyond the central one would be capable of arousing a sensation of light, so rapidly does the intensity of the bands fall off after the first.[45] Indeed, when we have created conditions most favorable to the scattering and irradiation theories, phenomena characteristic of the blind-spot fail to occur and the predictions we should make on the basis of the information they give us fail to be verified in experience. On the other hand, after ruling out irradiation and scattering, we still find vision in the blind-spot.

(*4*) *Contrast theories.* Contrast theories owe their origin to the belief that the positive phenomena seen in the blind-spot arise from contrast. They imply that we never see in the blind-spot unless neighboring regions of the retina or their projections in the visual areas in the brain are stimulated. Charpentier, Brückner, and Tschermak have formulated contrast theories having points in common yet fundamentally very different.[46] The chief point of difference between these writers lies in the fact that Brückner places the contrast mechanism in the brain and assumes physiological irradiation in the retina as a bolstering hypothesis, while Tschermak leaves the question open as to the exact mechanism at the basis of his theory and tries to dispense with irradiation. Tschermak requires, as a bolstering prop to his theory, psychic completion activities to account for the filling in of the blind-spot. Brückner, following Charpentier, believes that the central projection areas of the blind-spot are represented by ganglion cells to which we must look for an adequate mechanism responsible for the contrast effects in the blind-spot and which the neurone and fibrillar theories cannot explain. In assuming that the optic disk must be represented in the brain by ganglion cells even though it is devoid of end-organs Brückner recognizes the positive part played by the optic disk in vision—a most important admission from our point of view. Of special interest to the psychologist in this theory is the assumption that all contrast according to Brückner must be *central*. But both Brückner and Tschermak need special assumptions to explain why the blind-spot is invisible after having explained why it becomes visible through the action of their contrast mechanisms released under special conditions. The first assumes that local adaptation and physiological irradiation work against visibility of the blind-spot, while the second leans too heavily upon the assumption of a retinal or retro-retinal *Eigengrau* whose existence is being disputed more and more in the light of recent experimental evidence. Once again we find irradiation-diffusion theories used to explain opposite facts; now irradiation is postulated to explain the visibility of the blind-spot (Charpentier) and again it is used to explain the invisibility of the blind-spot (Brückner). Such a theory can serve no useful purpose.

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[43] Cited by Helmholtz, *ibid.*

[44] Weber, *op. cit.*, *Ber. d. k. Sächs. Ges. d. Wiss.*, 4, 1852, 185.

[45] We take this point up again in the last section.

(*5*) *Physical theories.* Both the physiological and the physical theories proposed to explain the visibility of phenomena in the blind-spot assume that what is seen there depends somehow upon the action of regions surrounding the blind-spot, and hence not upon direct stimulation of the optic disk itself. The difference between the two types of theory has already been pointed out: the physiologic theories assume that neighboring regions around the blind-spot respond as a result of irradiation from one retinal element to another. The reasons for their untenability have also been given. We are left, then, with purely physical theories unless we are content to invoke the aid of unknown mysterious psychic factors like *Einbildungskraft*, *innere Wahrscheinlichkeit*, and *das geistige Auge*. Briefly summarized, the physical theories assume that when light falls upon the optic disk or is directed toward it some light is scattered through the media of the eye or along the optic nerve fibers by refraction or through the formation of diffraction bands (blur circles) and reaches the sensitive retinal elements near the optic papilla. This effect may include the numerous phenomena comprehended under the terms halation, dispersion, diffusion, diffraction, chromatic aberration or irradiation in the larger sense of the word. All of these factors conspire to make the eye a far from perfect optical mechanism even in perfect accommodation where point-like images are not delivered to the retina from point-like stimuli. Helmholtz apparently includes all of the effects of scattering and the like under his phrase "the formation of blur circles on the retina." We have next to ask if blur circles can account for vision in the blind-spot and have already partly answered the question when we pointed out that the blur circles would have to extend several hundred times the diameter of the first diffraction band to be effective and that probably no band with medium intensity of light is visible beyond the second. Several principles apply in the formation of blur circles: (1) they are proportional in size to the intensity of light; (2) they are relatively greater for small objects than for large, being in fact practically negligible for large objects; and (3) the diffraction effect is greater for the long wave-lengths than for the short, though in dealing with such small magnitudes in comparison with the total area of the blind spot we may treat them as equal. In our theoretical discussion we shall consider the question whether diffraction bands or blur circles are sufficient to account for what is seen when we consider the quantitative aspects in more detail.

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[46] Charpentier, *C. r.*, *Acad. Sci.*, 126, 1898, 1634-1637; Brückner, *Arch. f. d. ges. Physiol.*, 136, 1913, 615-620; Tschermak, *Erg. d. Physiol.*, 24, 1925, 330-378.

CONCLUSIONS FROM THE HISTORICAL SURVEY

That something is seen *in* the blind-spot cannot be open to question any longer in the light of many observations by numerous investigators. But even this fact has been denied by some writers. This state of affairs springs from three sources of error: (1) a very slight change in conditions, many of which are not understood, changes the observed phenomena so much that different investigators believe they have found absolutely different results; (2) hasty generalization from observations under limited conditions to the absolute capacity of the optic disk under all conditions; and (3) neglect of important and oftentimes crucial data which do not fit with one's own observations or theory.

Since practically all investigators and writers have united against Pecquet, Volkmann and the very few who have followed them in their belief that only the region of the central artery and vein is insensitive to light stimulation, we may say that while opinion is divided on the question of vision *in* the blind-spot, it is practically unanimous against the view that we see *with* the blind-spot, or in other words, that the optic disk somehow responds to light without the aid of adjoining regions. The crucial questions and the tests necessary to answer them now seem to the writer to concern direct stimulation of the optic disk. We must determine anew what is seen in the blind-spot and devise crucial tests of the hypotheses which deny any functional value to the optic disk itself.

ditions and with all *Os* makes the same claim for consideration that an unique quantitative finding would.

Methods of determining the boundaries and stimulating the blind-spot. The *O* sat facing a black cardboard screen 1 m. sq. in which a small cross had been cut to serve as the fixation point. Most of the experiments were conducted with the *Os* at a distance of 131 cm. although for some experiments the distance was changed to 4 m. Only the right eye was used in observation, the left being closed. A head-rest and biting-board made by taking a wax impression of the teeth on a metal base, all held solidly by rods and clamps to an upright bar on a heavy metal stand, served to keep the *O* rigid during observations. The *O* rested between observations, and experimental periods were not prolonged beyond the limits of ordinary fatigue in visual work.[47]

The boundaries of the blind-spot were determined in the usual fashion in a room lighted by a 150 w. incandescent lamp by bringing a white stimulus or test object (1 cm. sq. in size) into the field of vision until it disappeared while the *O* was fixated on the red cross at the left of the screen. The points of disappearance were marked along eight meridians at 45°. Only the places of *disappearance*, and not the averages of appearance and disappearance as is usually done in mapping the blind-spot, were taken as defining the blind-spot as we wished to be sure we were working well within the limits of the *macula caeca*. All stimuli exposed within the boundaries of the blind-spot were well away from the edges: when the observer sat 4 m. from the screen and we used a 2 mm. sq. light as the stimulus object, the diameter of the stimulus in this case was 1/200 the smallest diameter of the blind-spot which at this distance averaged 40.63 cm. for all *Os*. Usually the stimulus was an incandescent lamp of 150-300 w. behind a ground glass in front of which the black cardboard was placed. With small stimuli or patterns cut out of the cardboard fairly sharp figures could be obtained even with the ground glass. Experiments in which the source of light was directly behind the opening in the cardboard screen gave results similar to those with the ground glass interposing. It will be seen that in the main our experiments were conducted with the usual order of conditions reversed: instead of working with the retina illuminated, we worked in general in a dark or semi-dark room and *illuminated the blind-spot*, the rest of the field of vision receiving no stimulation so far as general room illumination or the presence of other stimuli was concerned, except when we wished to test the effect of the latter upon vision in the blind-spot.

Special checks against scattering and diffusion were made in ways which will be described later.

To control the intensity of our stimuli we used rheostats in series with the incandescent lamps. For weak and small stimuli small flashlight lamps and automobile headlight lamps supplied by batteries were used. Special means of dimming our stimuli consisted in covering them with white bond paper or filters.

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[47] J. N. Evans, Angioscotometry, *Amer. J. Ophth.*, 9, 1926, 489-506, has succeeded in mapping the larger vessels of the retina. He has found two hours' mapping not excessive so far as fatigue of "fixation" goes, but stresses the possibility of "mental fatigue." We seldom worked for more than an hour with an *O*.

We varied the color of our stimuli by the use of ordinary gelatine filters pasted over the patterns cut in the cardboard screen. The light transmitted by our filters varied with the changes in intensity of our source lights, as would be expected. At medium and low intensities the green showed traces of blue while at high intensities of light it became yellowish. The blue filter was consistently blue except at high intensities when it lost so much in chroma as to be rather whitish. The red filter gave a bluish-red color at medium and low intensities but by using two thicknesses we could get a very good red from it. The amber filter was the least satisfactory, giving a reddish-brown color at low intensities and a reddish-yellow at high. The source lights without the use of filters, like ordinary incandescent lamps, were fairly yellow at medium intensities, brownish-yellow with low intensities in direct vision, and bright yellow, almost white, at high intensities. This account does not pretend to give the spectral transmissions of the filters or their specifications according to any other mode of physical analysis—we merely wish to convey how the lights *looked* with various filters at various intensities—for our purposes it is sufficient if we compare foveal and peripheral vision with vision in the blind-spot.

RESULTS: I. SIZE

The sizes of the blind-spots of our *O*s were found to be as follows: *B* (Mr. S. H. Bartley, graduate student and instructor in psychology) 5° 40′; *G* (Dr. J. P. Guilford, assistant professor of psychology) 6° 41′; *M* (Dr. Beulah M. Morrison, assistant professor of psychology) 5° 30′; and *W* (Dr. R. H. Wheeler, professor of psychology) 6° 4′. The horizontal diameters of the blind-spots of these *O*s at 131 cm. averaged 15 cm. and were slightly less than the vertical diameters because the shape of the blind-spot is decidedly more elliptical than circular. These values for the blind-spot compare well with those found by other experimenters though we must note that they are slightly smaller than if we had taken the average of *in* and *out* determinations as the boundaries of the blind-spot.

When we decided to make special test of the blur circle and diffraction hypothesis we worked within the projection of the blind-spot at 4 m. distance thereby getting nearly three-fold enlargement which enabled us to work with an extraordinarily large ratio of stimulus to blind-spot. A stimulus at 4 m. distance that measures 3 mm. on a side will be approximately 1/135 the diameter of the average blind-spot (40.63) leaving as great a margin of safety for blur circles, irradiation, and scattering as could possibly be desired. In terms of area this stimulus had 1/210 the area of the blind-spot (40.63 × 46.71 cm.). Such a stimulus placed at the center of the blind-spot was easily seen by all observers and in a few observations we made our stimulus as small as 2 mm. on a side with nearly as good results!

Two methods of mapping the blind-spot deserve mention at this point because of the unexpected results they gave. The fact has already been mentioned that the blind-spot shows different contours for stimuli of different colors and brightness. If, now, we map the blind-spot with the room in darkness, using as our stimulus object or 'prober' a small automobile light carrying from 4–6 v. and encased in a black box so that only a very small beam is emitted, we find that *there is no blind-spot.* The light will be seen throughout the region previously unresponsive to our white prober when the room was normally illuminated as in our previous method of mapping. For 2 *Os* there were two tiny spots within the so-called blind-spot where the light disappeared. Another way in which to obtain the same effect is to cut a slit the length or width of the blind-spot in the screen lighted from behind and to pass another screen having a smaller slit of any desired size over the first, thus exposing a stimulus throughout the length of the blind-spot in any meridian. As the light passes the edge of the blind-spot obtained in ordinary mapping it loses its form, and shows a bright central core surrounded by a halo or haze of light which may or may not reach as far as the boundary of the blind-spot. Whether what is seen is due to stimulation of adjoining regions it is the purpose of this paper to answer.

We then reversed conditions. The room was kept in darkness as before but the whole visual field was lighted by removing our black cardboard from in front of the ground glass. This was done in accordance with a suggestion made by Professor Fernberger. A 300 w. incandescent lamp served as source and lighted the whole field very strongly. Using now a black stimulus as prober, the *Os* reported where it disappeared. The black object disappeared just as the white prober did before and we obtained approximately the same outline and area for the blind-spot as before. In this case the light which had every chance to diffuse into the blind-spot did not fill it in any sense whatsoever, because, while the *Os* did not always see the blind-spot as a gap in the field, the presence of the prober made no difference in what was seen—the opposite being true when we interfere in any way with a light that directly illumines the blind-spot when the rest of the field is dark. Sometimes, under these conditions the *Os* did not see a homogeneous field, as Helmholtz and Werner insist is the case, but reported a gap in the field corresponding to the projection of the blind-spot. In these cases, *light on all sides of the blind-spot failed to irradiate and complete the gap in the field as required by the traditional theories.*

From the results of our mapping we may say that the size of the blind-spot depends upon the intensity of the stimulus used and the intensity of the surrounding field. When the brightness difference is in favor of the blind-spot it may be made as small as we please, even to the point where the stimulus does not disappear at all, or with some *Os* vanishes in a region so small as hardly to coincide with the true anatomical disk.

Having drilled our *Os* in holding their fixation, reporting eye-movements, winks, or any other disturbing factors, we instructed them to keep their eyes closed between observations and to open and fixate the right eye upon the red fixation spot when we were ready to expose a stimulus. When *O* had fixated and felt steady, *E* gave a 'ready-now' signal and turned on the light which illuminated the patterns we had cut in or outside the projection of the blind-spot. The instructions to the *O*s were simply to report whatever was seen. Sometimes additional instructions called for reports on special aspects of the phenomena, such as form, color, after-images, or the like. In no case was a question asked concerning a detail until it had first been reported by an *O*, in which case the *E* asked for more information in order to keep full records and make sure of crucial points.

All of the experiments were carried on at the University of Kansas and extended over a period of two years. While only four people acted as regular observers besides the writer, several others took part in the experiments, some of them from the physiology and biology departments, others students in psychology. At this place I wish to express my thanks and acknowledge my debt to my colleagues, Professors Wheeler, Morrison and Guilford, and Mr. Bartley, without whose coöperation, many suggestions, and helpful discussions this study would have been impossible.

II. QUALITATIVE CHARACTERISTICS

If a stimulus is exposed in the projection of the blind-spot it will be seen if it is sufficiently intense under all conditions. If the retinal field is dark, the stimulus falling within the blind-spot need not be so intense in order to be seen. But the illumination on the blind-spot must *generally* be greater than on the surrounding field, although not always as we shall see. What is seen is therefore a function both of the light affecting the optic disk and of the rest of the visual field. For this reason it is best and simplest to work in a dark room so that the stimulus in the blind-spot will be favored without special aids. After the experimenter has become familiar with the intensity of stimulus necessary to produce given results with given *O*s and the *O*s have become practiced in fixating one part of the field while attending to what goes on in the blind-spot, it is possible to expose two or more stimuli simultaneously so that the *O* will be able to see them.

When a single stimulus, say a small bright spot 2 cm. in diam., is exposed within the projection of the blind-spot, mapped as we have described in our first method, the *O* will see a diffuse light with a brighter, denser core in the blind-spot, usually the color of the stimulus if it is of medium or high intensity. If the stimulus is of very low intensity it will change in color and finally disappear, in prolonged observation often leaving an after-image, positive or negative. Stimuli in the blind-spot lack definite form unless special precautions are taken to determine the form in a manner to be described later. The bright light seen may be described as flat, hard, or surfacy as against the diffuse halo effect surrounding it. Several *O*s described the light as being in three dimensions and shaped like a cone coming toward them.

If a very small light source is used to explore the region of the blind-spot there will be found places more sensitive than others; in one place or two the light may disappear entirely, but the area of this region of absolute blindness is so much smaller than the total area of the optic disk as to make it doubtful if the statements generally made concerning the extent of the blind-spot and its anatomical locus are true. In fact the region of blindness corresponds rather to the entrance of the retinal artery and vein than to the whole of the optic disk.

All colors will be seen in the blind-spot under the conditions of stimulation described. After-images, contrast effects, adaptation, and many other phenomena characteristic of normal vision will be found in the blind-spot, only in somewhat different degree, rather easily by the method of procedure we adopted, viz. illumination of the optic disk while the rest of the field is dark. At this point we wish to stress the fact that the phenomenon typically seen is a bright central core with diffuse halo, when one stimulus is exposed in the blind-spot; when several stimuli are exposed there may be several 'cores' corresponding to the light sources, or no core at all.

III. Apparent Size of Stimuli

The apparent size of any light stimulus falling on the blind-spot will be affected by three factors: (1) the size of the stimulus; (2) intensity or brightness of the stimulus; and (3) the duration of the stimulus. For small stimuli the spread of light is less than for large, although any light tends to spread throughout the whole optic disk. *O*s judge size both by the amount of spread and the intensity, the former being a direct function of the latter. The second factor is of greatest importance since both the brightness of the central core and the size of the halo depend upon this factor. All the *O*s report that the total size decreases as the intensity of the source is decreased.

Noticeable changes occur as a stimulus is exposed in time. In the first moment of exposure of a stimulus, a small light is discerned by *O* which rapidly grows larger attaining a maximum for a moment, after which it may shrink to as much as one-third, staying thus for many seconds. This initial growth and shrinkage is accompanied by *gamma* movement or constitutes it and is easily seen as one exposes an ordinary light momentarily.[48] As adaptation proceeds the light grows smaller and smaller, changing in color until it disappears entirely. The rate of growth and shrinkage both depend upon the intensity of stimulus. *O*s adapt so quickly to faint stimuli they may last only for a few seconds in vision.

It is, of course, possible to adjust the intensity of the stimulus so that the total spread of light does not reach to the boundaries of the blind-spot as the following experiment shows: a circle of spots, 1.25 cm. in diam. is exposed around the edge of the blind-spot and a single spot 2.5 cm. in diam. is exposed within the blind-spot as shown in Fig. 1. The *O*s report that between the outermost edge of the halo within the blind-spot and the circle of lights surrounding the blind-spot there is a dark space with no light. In this case we can be sure that the light diffused or scattered in the blind-spot does not reach beyond it, hence what is seen cannot be due to neighboring retinal elements.

IV. CONTRACTION AND DISTORTION

If two spots of light in a darkened room are exposed on either side of the blind-spot (as in Fig. 2) all the *O*s agree that the spots appear nearer together than when seen in either foveal or peripheral vision. They will be seen as farthest apart in the fovea, nearer together in the periphery at the same distance as the blind-spot is from the center of vision, and the nearest together when one falls on one side of the blind-spot and the other on the opposite side. In the last case they may be only one-half as far apart as when viewed in direct vision. We have thus verified the findings of Ferree and Rand on this point. *We also found that for any part of the retina, the less intense the stimuli the nearer they seemed be to each other, and the more intense the stimuli the farther apart they were judged to be.* Comparing the distance between two as a square which rapidly changed to a circle, and then became a diffuse brightness with a brighter central core, as shown in Figs. 7, 8, and 9. All observers agree that as time goes on whatever distinctness of form the stimulus may have had is lost and the light gradually grows less bright and changes in color. The last color seen with continued observation (*Dauerbeobachtung*), as one adapted to the stimulus, regardless of what color filter was used, was *blue*. This is also true of the weakest intensity of light visible in the blind-spot, as reported in the section concerning changes in intensity of stimulus.

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[48] Shrinkage of the optic disks has been noted by Brückner, *op. cit.*, who ascribes it to the fact that surrounding retinal zones receive less stimulation after the first excitations. It is rather a special case of the rapid growth and decline of momentarily exposed stimuli which can be seen when any ordinary electric light is turned on and off quickly. Koffka and Kenkel have called this *gamma movement.* (For an account of this phenomenon, see my article, The Psychology of Gestalt, this JOURNAL, 1925, 36, 504). U. Ebbecke, Ueber das Augenblicksehen, *Arch. f. d. ges. Physiol.*, 185, 1920, 181-195, discusses the growth and disintegration of momentarily exposed light stimuli, pointing out normal vision is maintained between the two processes.

When the stimulus has completely faded out in long exposures, after-images often appear. In such cases the *O*s reported that the stimulus light had been changed. Two *O*s (*B* and *W*) often expressed uncertainty in their protocols as to whether the stimulus had been changed or they were seeing after-images.

Two other changes, besides loss of form, in time are important. When the light is first flashed on the stimulus expands from a small light to a maximum and quickly shrinks in less than a second to the size characteristic of its intensity and objective size. These three phases, really constitute what has been named *gamma* movement. After these initial phases the light gradually decreases in size and brightness, obviously a result of adaptation.

Adaptation occurs very much more rapidly in the blind-spot than elsewhere in the field of vision. Not only does the light seen decrease in size and brightness in continued observation but undergoes changes wholly analogous to the changes observed as the intensity of light is decreased. Green goes directly to blue as the *O*s adapt, yellow becomes more reddish and finally blue, amber becomes more brownish-red and finally blue. The last color seen either as a result of adaptation, or of decreasing the intensity of the stimulus from noticeability to unnoticeability or increasing the intensity of the stimulus from unnoticeability to noticeability, is blue. While this might be regarded as a result of chromatic aberration wherein the shorter wave-lengths attain greater effectiveness for vision in the blind-spot, it is very doubtful whether this is the explanation. Whether the optic nerve fibers are more sensitive to short wave-lengths or we are dealing with something analogous to the blue arc phenomena, we hardly dare venture an opinion. The fact that Ferree and Rand found the blind-spot to be smallest when mapped with a blue stimulus has been noted above.

VI. Effect of Intensity

The effect of changes in the intensity of the stimulus on phenomena in the blind-spot was next considered.

Procedure. The intensity of the stimulus light exposed in the blind-spot was varied by means of rheostatic control of the current supplying the sources of light. The *O*s reported changes as the stimulus was changed from dim to bright and from bright to dim. Red, green, blue and amber filters were used.

Results. At the weakest intensity, regardless of the color of the stimulus in foveal vision, the light in the blind-spot was seen as blue. Typical reports like the following substantiate this generalization.

No filter; *E* decreases intensity of stimulus; *O* reports: "At first a yellowish white light which gets redder and redder, than an azure blue, shrinking in size at the same time."

Green filter; procedure as above: " A green light which gets bluer and bluer until there is no green left in it."

Amber filter; procedure again like above: "The light was yellowish-red at first, then became redder, then blue at the end."

With the blue filter decreasing intensity changes the size of the light seen, with only a darkening of hue at the lowest intensity.

If the stimulus is exposed at the lowest possible intensity and gradually is increased, the reverse order of appearance of colors with each filter is found. All filters look blue at the lowest intensity. The amber filter changes from blue to red or orange and only at high intensities does it appear with sufficient yellow in it to be called amber. The red filter begins as blue, although it looks a good red in direct vision, turns to red and gains in yellow as the intensity is increased. The green filter requires less light in order for the green component to become visible. At highest intensities the green filter looked yellowish.

We next compared the order of colors as the intensity of stimulus changed when the stimulus fell within the blind-spot and when it fell on the edge of the blind-spot. Another fixation light was so placed that by moving the eye from the usual fixation-point the stimulus could be made to fall upon either the right, left, top or bottom edge of the blind-spot. It was thus possible to compare what was seen in the blind-spot at any given intensity with what was seen on the edge immediately contiguous to it. The results are most important for those theories that assume what is seen in the blind-spot is due to stimulation of neighboring retinal elements. *For a stimulus of given intensity which appears in the blind-spot as blue appears on the edge of the blind-spot as bright yellow and whereas the color of all stimuli at lowest intensities in the blind-spot is generally blue, on the margin of the blind-spot it was found to be yellow with no filter, and orange with the amber filter.* On the basis of the scattering theories of vision in the blind-spot we should expect the first visible light on the edge of the blind-spot to be the same as the first visible one within, but this does not happen. Nor can it be said that what is seen in these cases in the blind-spot is an after-image because; (1) it holds for the first stimulus seen as the intensity of the stimulus is increased; (2) the colors seen do not necessarily conform to the color of the negative after-image, nor is it the positive since the intensity of the stimulus is on the increase; and (3) obliteration of the stimulus light may be followed by an actual after-image, different from the previously reported light, or by no after effects at all, showing that what was seen does not fall under the category of those phenomena classed under persistence of vision.

The effect of change in intensity of stimulus upon size of the light seen in the blind-spot has already been noted in Section III. It was pointed out that due to spread of light in the optic disk all stimuli tend to be seen as diffuse patches of light larger than the stimulus. Since we should expect the spread or diffusion of light to be proportional to intensity we have an explanation for the generalization that the greater the intensity of stimulus, the larger the patch seen in the blind-spot. With increase in intensity go also an increase in size and brightness as well as the changes in color noted above.

But the intrinsic brightness of the stimuli falling in the blind-spot is not alone of importance in what is seen there. If observations are made in a lighted room, rather than under conditions of darkness as most of our experiments were made, then the effect of lighting the rest of the retina is equivalent to decreasing the intensity of the stimulus in the blind-spot. For the brighter the room light (or the rest of the retina), the brighter must be the stimulus falling in the blind-spot in order to be seen. Thus a stimulus which can not be seen in the blind-spot while the room light (150 w. incandescent lamp, shielded from the *O*'s face) is on, will begin to emerge in the blind-spot more and more strongly as the room light is turned down.[50] It will attain its maximum brightness and size when the room is completely darkened. *Maximum effects in the blind-spot are obtained when the difference in brightness between the blind-spot and the rest of the retina is a maximum and in favor of the blind-spot.*

If two stimuli are exposed simultaneously within the blind-spot, only the stronger, or in some cases the one having favored position, will be seen. But if the intensity of the two be made equal, then both may be seen at once, or rivalry may set in (see Section IX).

In concluding this section the fact must be emphasized that all phenomena in the blind-spot depend in large measure upon the intensity of the stimulus, the state of adaptation of the eye, and the degree of illumination of the field outside the blind-spot.

VII. EFFECT OF ADAPTATION

The phenomena accompanying the processes of adaptation in the blind-spot are here described.

Procedure. Stimuli of various sizes and intensities were exposed well within the projection of the blind-spot, the *O* being told to report the changes in time.

Results. As in the case of normal vision, if a stimulus is fixated for a sufficiently long time without eye movements it will change in hue, chroma, and brightness. Weak stimuli change very quickly in the blind-spot, rapidly becoming blue and finally disappearing altogether. Changes in the blind-spot occur very much more rapidly than in any other part of the visual field. An eye movement or wink may restore an invisible stimulus to visibility. *O*s differ as to the speed with which they adapt in the blind-spot, one *O* in particular adapting most rapidly so that weak stimuli were visible for only a few seconds at the most.

A typical description of the course of the phenomena is as follows: "At the beginning an orangeish-red patch with tiny core. [The amber filter was being used]. As I looked it became dimmer and the core remained as a central point. After a while it got smaller and redder, then blue, a very washed out blue."

After the stimulus had completely disappeared so that nothing was visible in the blind-spot the observer shifted his fixation to another point so that the stimulus then fell on one of the edges of the blind-spot. Immediately the stimulus reappeared in its original color. This shows that what has become a subliminal stimulus in the blind-spot is supra-liminal outside. Moreover, while the last color seen in the blind-spot is generally blue, outside the blind-spot it is generally white or gray before complete disappearance in adaptation. It thus appears that the blind-spot is a region of lessened sensitivity rather than 'blind' and possesses characteristic functional peculiarities which cannot be ascribed to other parts of the eye.

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[50] Feinberg, *op. cit.*, *Psychol. Forsch.*, 7, 1926, 16-43, reports a similar finding and gives some good illustrations of the effect of general illumination on what is seen in the blind spot.

The process of adaptation is in many ways exactly analogous to the effects following a decrease in intensity of the stimulus. As the blind-spot adapts the order of color changes follows the order of colors reported as the intensity of the stimulus was decreased.

If two stimuli are exposed within the blind-spot and the intensity of each regulated so that both are visible one may adapt out more quickly than the other. Thus when red and green were exposed, the red turned blue and disappeared, leaving the green. If one stimulus is within the blind-spot and the other outside, the one within the blind-spot usually disappears first, whether because of adaptation or inhibition we cannot say. Usually other mechanisms are at work to interfere with any clear-cut results so far as any one phenomenon is concerned, especially when the conditions are complicated through the introduction of other stimuli into the field.

One fact reported by one *O* requires special notice. *W* found that in continued observation the core was the first to adapt out leaving the surrounding haze or halo after its disappearance. But in other experiments where we obliterated the core by covering the center of the source light the halo disappeared with the core. This fact by itself cannot therefore be used in favor of the scattering theories since just the opposite has been found to be true when direct control of core and haze was undertaken as reported in the section on the irradiation theories.

VIII. Perception of Direction

The study of the perception of direction in the blind-spot, both of movements and of positions, was next taken up.

Procedure. A small spot of light was moved either vertically or horizontally in the projection of the blind-spot and the *O*s reported the direction of movement. While the experiment was originally designed to find out if there were differences in sensitivity to light in the various regions of the blind-spot, the interesting fact emerged that one *O* generally and other *Os* at times were wrong in their reports on the direction of movement. In this series of experiments *O* sat 4 m. from the screen, making the projection of the blind-spot average 40.63 cm. in diam., which, with a stimulus of only 4 mm. on a side gave ample room for exploratory movements. Sense of direction was also tested in this series by exposing a stimulus first, say, near the upper edge of the blind-spot and then another in the middle or lower portion of the blind-spot either leaving the first one present also or extinguishing it upon the appearance of the second. In these cases the *O* was required to report whether the second stimulus was below, below right, above, above left, etc.

Results. For *B* when the stimulus was moved from top to bottom of the blind-spot it seemed to be going downward but obliquely.

G had no difficulty in reporting correctly both the direction of movement of the stimulus and the spatial relations of the second stimulus with regard to the first.

M and *W* found the most difficulty in judging directions. For *M*, stimuli exposed in the center of the blind-spot seemed to be on the right and stimuli at the top were reported on the left. *M* was not consistent, however, in her reports, sometimes reversing this type of error. This may have been due to the fact that she was able to see either not at all or poorly in the very center of the blind-spot. When stimuli near the top and center of the blind-spot were exposed and the center one extinguished, *M* reported that the light was being extinguished from right to left (the two within the blind-spot were not seen as two but as one large patch).

W also called the central stimulus left and the upper right, thus reversing *M*'s illusion. When asked to draw the relative positions of the two stimuli, the central one was drawn below and to the left of the upper one. In these observations both stimuli were exposed simultaneously and then the center one obliterated. When one stimulus was exposed alone, *W* had no difficulty in placing it at the top or bottom, just as it was really placed in the projection of the blind-spot.

In the case of both *M* and *W* we must ascribe these displacements to differences in sensitivity within the blind-spot and not to the stimulation of sensitive retinal elements because the stimuli were equidistant from all edges of the blind-spot, and in cases where they were not, were not displaced towards the nearest edge as they should be if the scattering and irradiation theories are correct. We shall discuss this point at greater length in the section on irradiation where similar experiments were carried out for the express purpose of settling this point.

IX. DOMINANCE, RIVALRY, AND FUSION

The effect of different stimuli, both within and without the blind-spot, upon each other was next considered.

Procedure. In this series of experiments more than one stimulus was exposed at a time, either two in the blind-spot or one within the blind-spot and one or more outside the blind-spot near one of its edges. All combinations of the colors red, green, blue, amber, and yellow, taken two at a time, were used.

Results. Just as in normal vision when several stimuli are exposed simultaneously in the visual field, one of four things may happen: (1) all the stimuli may be seen simultaneously; (2) first one stimulus may be seen, then another, in rivalry; (3) one stimulus may dominate, obliterating the other so completely it never appears; (4) or the stimuli may fuse. All of these possibilities were realized with all observers in the blind-spot.

Dominance. This was the easiest to get because if either stimulus was stronger than the other, whether within or without the blind-spot, the other did not appear; although it must be borne in mind that if the stimulus in the blind-spot is to be seen as against the one on the edge it will have to be a great deal more intense than if the one on the edge is to dominate over the one in the blind-spot. This is in accordance with our law that if anything is to be seen in the blind-spot the differential brightness must be in favor of the optic disk. Some typical results follow.

B reported only the blue when blue and red rectangles (23 mm. × 3.5 mm. with 45 mm. between) were exposed within the blind-spot (Fig. 10). Nor was the red seen in this case after the blue had adapted out and nothing was seen. This was true of all the *O*s until careful adjustment of the intensities made it possible for them to see both simultaneously.

G reported only the two yellow rectangles on either side of the blind-spot when they were exposed with a blue rectangle inside the blind-spot (Fig. 11). Domination of the stimulus in the blind-spot over the stimulus outside was obtained by making the blue in the blind-spot very intense and an annulus of yellow around the blind-spot very weak (Fig. 12). *O* reported a very dark navy blue with almost no trace of the ring outside in the last case, which shows that a stimulus inside the blind-spot may dominate over one outside.

M and *W* report domination of one stimulus over the other when both are within the blind-spot (Fig. 13). In this instance the stimulus falling in the center of the blind-spot was effective and the addition of another stimulus near the top edge of the blind-spot made no difference in what was seen. If, however, the top stimulus was exposed alone it was seen, but as soon as the center stimulus was added it blotted out the top one. It should be borne in mind that here the 'top' stimulus was 15 cm. from the nearest edge of the blind-spot while the central one was 25 cm. and hence, on the irradiation and scattering theories, the 'top' one should have been preferred since the amount of light affecting neighboring sensitive retinal elements is inversely proportional to the distance from them.

A somewhat different type of dominance is that reported by *W*. Three squares were exposed as in Fig. 14, one above the blind-spot, the second in the blind-spot and the third below the blind-spot. When all three were exposed simultaneously the one in the blind-spot was not seen at all. But when either the top or the bottom stimulus was obliterated the one in the blind-spot appeared. But when the third square was added to the other two, the middle one disappeared.

Rivalry. Under conditions described above, a more or less regular alternation of stimuli may occur, thus when red and green (Fig. 10) were exposed within the blind-spot first one was seen and then the other. By making now the one color more intense, now the other, either could be made to obliterate the other.

A good description of the fluctuations is the following given by *G* when a blue was exposed in the left half of the blind-spot and a red rectangle in the right half as shown in Fig. 10: "First a good rose red at right hand side, not very large and not very dense. Then a big patch of blue loomed up more to the left with a strong dense center and a lot of diffusion to the left. Red disappeared for a moment and came back at the right. Several minor fluctuations in which first blue would appear and then red as in retinal rivalry."

Similar descriptions of rivalry within the blind-spot were given by all the *Os*.

Fusion. Occasionally when two differently colored stimuli were exposed simultaneously within the blind-spot they fused and were seen as a mixture. *G* reported when red and green were exposed that he saw a gray. This is the only case of fusion involving complementaries with a resulting gray. Sometimes when there is not a good dividing line between the two colors they may merge into each other, thus red and yellow gave orange either over the whole field or in the region between the two stimuli, while the red and yellow were seen as such beyond the orange region.

X. EFFECT OF DUAL STIMULATION

Dual stimulation was most difficult to obtain due to the fact that if either stimulus is brighter or more *Eindringlich* for any reason it tends to dominate or rivalry sets in. With careful adjustment of the intensities of the two stimuli it was possible, however, for all *Os* to see *two* colors at the same time when both were exposed within the blind-spot.

Procedure. Different pairs of colors, red and green, green and blue, blue and yellow, red and yellow, yellow and green, and red and blue were exposed within the limits of the blind-spot as shown in Figs. 10 and 15. The stimuli were either rectangles cut in the cardboard screen 23 mm. × 3.5 mm. with 45 mm. between, illuminated from behind, or two concentric rings of different color. The *Os* were not aware of the colors or of their position before they appeared. The position of the colors was changed, one appearing on the right, above or below the other. *O* reported not only on the color but its position and relation to the other, if both were seen.

Results. All the *Os* were able to see two colors simultaneously within the blind-spot. A typical description of the phenomenon, reported by *G*, follows: "I saw two colors, red at the right and green-blue at the left. There was a good dividing line between them, *i.e.* where one left off and the other began."

We next utilized a test suggested by Professor K. M. Dallenbach and Dr. W. T. M. Forbes of Cornell University as crucial for the theories of irradiation and scattering: two concentric rings, both within the blind-spot and of different colors (see Fig. 15) were exposed. In this series of experiments the blind-spot was 20 × 27.5 cm., the red ring having a diameter of 12 cm. and 1 cm. wide while the inner ring was blue having a diameter of 4 cm. and was solid. There was therefore a distance of 7 cm. between the inner color and the surrounding one and 8 cm. from the edge of the outer ring to the nearest edge of the blind-spot. In this procedure the outer concentric ring acted as an envelope for the inner and if they were both seen at once it is inconceivable on the basis of the irradiation theories how the inner could be seen without breaking through its envelope. Under these conditions 3 *Os* (including the writer) *saw both colors, simultaneously, one within the other:* one *O* was uncertain, and a fifth was unable to see both. In order for this experiment to succeed it is necessary to control the intensity of each color and to vary it until the proper intensity is obtained so that neither will dominate. The space between the two colors helps eliminate fusion. Momentary exposure may aid some *Os* to see both colors clearly simultaneously.

XI. AFTER-IMAGES

We describe in this Section the after-images that we obtained by the direct stimulation of the blind-spot.

Procedure: E exposed light stimuli in the projection of the blind-spot and *O* was instructed to hold his fixation after the stimuli disappeared and to report if any after-images appeared. The length of time of exposure of stimuli varied from a fraction of a second to several seconds. A variant of this procedure consisted in exposing stimuli which fell partly within and partly outside the blind-spot (Fig. 16).

Results. All the *O*s report after-images, positive and negative, from stimuli either falling wholly or partly within the blind-spot. In fact our first knowledge of the presence of after-images came not as a result of direct search for them but as a by-product of experiments designed for other purposes. We then made special experiments to detect the presence of after-images in the blind-spot. One *O*, *G*, who had special practice in another problem in observing the course of the after-image tried for a time to identify and follow its various phases (more than six in number). But apart from establishing the general fact that after-images in the blind-spot go through successive phases as in normal vision we were unable to control conditions with sufficient exactitude to offer anything certain in the way of identification of single phases. Several phenomena deserve special mention and will be given as originally reported.

When a long narrow rectangle extending from the left side of the blind-spot and through it to the right side (Fig. 4) was shown to *B* momentarily, he reported a chalky white rectangle in the after-image (stimulus light at high intensities was white with a yellowish tinge) not quite as large as the one originally seen while the stimulus was on, the edges having a diffuse outline. This is a case of positive after-image in the blind-spot. In the case of this after-image obtained with very short exposure of stimulus there was no more distortion of outline through the blind-spot than elsewhere—a fact which holds for direct stimulation as well if the stimulus is momentarily exposed.

The fact has been mentioned that all stimuli at low intensities or at very great distance and small appear blue in the blind spot. *B* reported that a faint yellow stimulus which appeared blue in the blind-spot changed to yellow (after stimulus was obliterated).

G reported two negative after-images from two stimuli exposed simultaneously in the blind-spot. In this experiment *E* exposed a red rectangle on the left and a blue rectangle on the right, both well within the blind-spot. *G* reported "first a blue patch to the right then a red patch to the left. Red and blue fluctuated. Then for a time both were present. At first there was a good dividing line between them which disappeared toward the last. When the black card was put up (*E* thus obliterated the stimuli, leaving the whole visual field dark) the left-hand side turned green and the right-hand side was very dark, yellowish green." (Our blue filter was not monochromatic, hence we might expect some other color added to the yellow in the after-image.)

M reported a green after-image from a red stimulus and a blue after-image from a yellow stimulus.

W reported an orange-yellow after-image after stimulation with blue. When the edge of the blind-spot was surrounded by an annulus of yellow and the center was stimulated by blue (Fig. 15) and the intensity adjusted so that *W* could see both simultaneously, upon closing his eye, *W* saw the annulus as blue and the spot in the center as yellow in the after-image which appeared.

After stimulation with white light, if a black screen was placed over the projection of the blind-spot and the room light turned on all of the *O*s reported the blind-spot as a black disk blacker than the background, plainly visible.

XII. PERCEPTION OF APPARENT AND REAL MOVEMENT

The perception of movement, apparent and real, in the blind-spot is now considered.

Procedure. A small light, 2 mm. sq., was moved about in the projection of the blind-spot as the real moving stimulus. For the production of apparent movement (stroboscopic or *phi*-movement) two methods were employed: (1) we found that if one stimulus was exposed briefly and then a second exposed in another part of the blind-spot it was possible to produce apparent movement, although in this simplified procedure it is not possible to produce movement with the regularity and smoothness we have come to expect as a result of the excellent methods developed since Wertheimer's classic study; (2) by cutting two holes in the screen on which the blind-spot was projected and by rotating a disk behind them in which slits had been cut so as to coincide with the holes in the screen it was possible to regulate the time of exposure of each stimulus in the blind-spot, the time and distance between stimuli and the intensity of each. By this latter means apparent movement from stationary stimuli can be produced at will, the direction and character of movement being independent of the direction of rotation of the disk since it is seen equally well in a direction opposite to that in which the disk revolves.

Results. As we have noted in Section VIII, real movement is perceived by all the *O*s in the blind-spot. Stationary stimuli exposed in rapid succession by the first method produced apparent movement for *G*. Since this *O* had been observing in a problem on the *phi*-phenomenon, he may have been 'set' for it and hence perceived it under conditions that were not optimal for it. Yet *G* was surprised to find movement under these conditions since we were at work upon another aspect of the problem. We thereupon repeated our procedure with another *O* (*W*) who also reported movement, but he was not so sure about its sensory character, feeling that he may have 'inferred' it. Our purpose in mentioning these two cases here is to show how easy it is to produce apparent movement in the blind-spot, even with such crude procedure as is involved here—procedure which would not suffice in direct vision. But other investigators have also pointed out the fact that the *phi*-phenomenon occurs even more readily and smoothly here than in other parts of the visual field.

With apparatus actually designed for the production of apparent movement, as described in the second method given above, we were able to substantiate the findings of Stern[51] and others that movement may be seen in the blind-spot (1) when both stimuli lie on either side; (2) when one stimulus is within, the other without the blind-spot; and (3) when both stimuli are entirely within the blind-spot. All the *O*s have no difficulty in seeing apparent movement in the blind-spot when the conditions are at all favorable.

The perception of apparent movement within the blind-spot when both stimuli lie within it is most significant for any theory of vision in the blind-spot. For it is inconceivable on the basis of the scattering and irradiation theories how light radiating from different parts of the optic disk to surrounding retinal elements could give rise to a movement process unseen in the sensitive regions and then localized as in the blind-spot. According to the irradiation theories we are required to assume not only that single *points* falling in the blind-spot are seen in the blind-spot through their effect on neighboring retinal points but, what is far more difficult to believe, that the retinal points stimulated give rise to a movement process over a retinal *area* (or line) unseen in the places actually excited but localized as in the blind-spot. The same difficulties will be encountered if, *mutatis mutandis*, the process is supposed to take place in the cortical projection of the retina. It was this same difficulty of localization in the irradiation theories which forced Helmholtz and others to give them up in part. In the face of these and other findings they must be given up *in toto*.

XIII. PERCEPTION OF FORM

In general, especially after the first fraction of a second of exposure of a stimulus, we may say that perception of forms in the blind-spot is either poor or wholly lacking. The usual thing seen is a haze spreading out in all directions, regardless of the form of the stimulus. But differentiations in brightness within the haze may lead the *O* to infer that certain figures are present. Thus triangle, circle, square and rectangle seem different even though the *O* cannot say just what the form of each may be. There are, however, two notable exceptions to the general statement that form perception is either lacking or poor in the blind-spot: (1) when the *O* *knows* the shape of the stimulus exposed he is able to assert that differentiations in brightness within the total field suggest what is actually the form of the source; (2) but with very brief exposures or with extreme care on the part of the *O* during the first moment of stimulation, the form of the stimulus is actually seen and not inferred. Forms may be seen with sufficient distinctness to be recognized and named correctly. After the initial *Einprägung* the form evaporates, as it were, into the diffuse phenomenon described above. Even though the process of diffusion *within* the optic disk takes place rather quickly it is still not so rapid but that the *O* can detect the form before diffusion has taken place and can actually see the change from a more or less clearly bounded figure to a large shapeless mass possessing more or less brightness differences within. One *O* (*W*) is positive that form is given *perfectly* at the beginning of stimulation, but that its rapid disappearance makes it hard to detect.

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[51] A. Stern, *op. cit.*

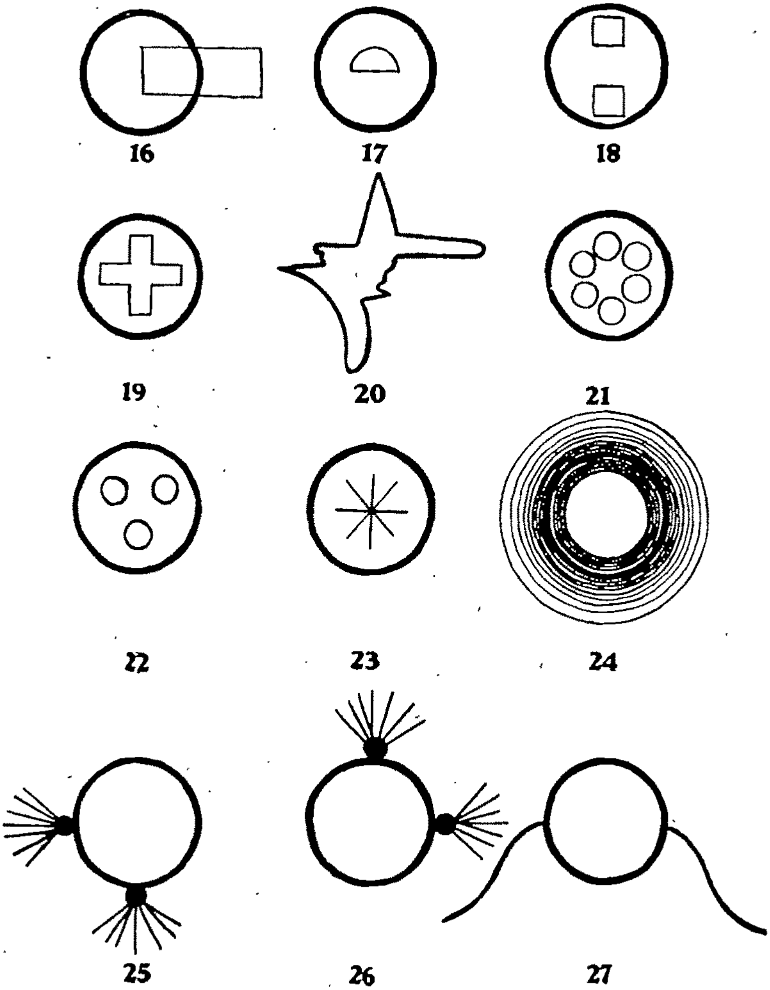
Procedure. Various figures at different intensities were exposed in the projection of the blind-spot with both short and long exposure times, with and without knowledge. The *Os* were required to report on the form of the phenomenon seen and to note any changes it underwent in time.

Results. Typical results for each *O* are given, (1) observing without knowledge and then (2) observing with knowledge.

(1) *Without knowledge.* *B* reported a half moon exposed in the blind-spot as "a ring effect, not fully complete" (Fig. 17). Two squares, one above the other, were reported as an upright rectangle (Fig. 18). In this case diffusion occurred *within* the blind-spot from one figure to the other over a distance greater than the distance from either stimulus to the nearest edge of the blind-spot, the total phenomenon being localized within the boundaries of the projected blind-spot. A cross exposed in the blind-spot (Fig. 19) was called a cross but its form was greatly distorted as compared with the stimulus. It appeared as shown in Fig. 20. Six small circles exposed in the blind-spot were reported as giving rise to a less homogeneous field than three spots (cf. Figs. 21 and 22).

M reported a triangle exposed with the base uppermost as a vertical slit with the lower end narrower than the upper. A number of slits forming an eight-rayed star were reported as differentiated, not round (Fig. 23). A cross appeared not exactly round but *M* could not say what shape it was. The two squares in Fig. 18 appeared as *two* spots for *M* in contrast to the single figure *B* reported.

W reported that a half moon was different from a cross, but could not tell in what respect. Multiple cores, as for *B*, are distinguished from one or few cores (Figs. 21 and 22).



FIGS. 16-27. For explanation see legend to Figs. 1-15 and the text.

(2) *With knowledge.* *G* and *W* looked directly at a blue rectangle and then turned the eye so that it fell in the blind-spot. Both reported that form was preserved even in the blind-spot, although not as definite and clear as in foveal vision.

G describes the experience as follows:

> "First I looked at fixation-point and got a blue patch of light in the blind-spot, rather small, formless, all a film color, no hard surface, and more dense in the center and much smaller in area than the blind-spot. As I turned my eye slightly off the fixation-point toward the blue stimulus, about two inches from the fixation spot the blue shrank in size, became smaller and elongated vertically and then seemed to be less blue and dense. Going farther it took its definite rectangular shape and got more surfacy and definite until I fixated it directly. Going back the changes took place in reverse order. When seen at the time my fixation was about two inches from the fixation point it seemed less blue than at the fixation point itself. After I knew the shape of the thing, while fixating the red spot (*i.e.* while stimulus fell within blind-spot again) it retained its elongated form."

W's reports show much more definite preservation of form in the blind-spot when working with knowledge.

> "After looking directly at the blue slit and then turning fovea to the red fixation light the blue slit kept its form."

W was unable to find a place in peripheral vision where an intense blue stimulus lost its form to any appreciable extent, showing that the amount of scattering of light after reaching the retina is so small as not to be perceptible, hence the diffuse nature of the phenomenon seen in the blind-spot is due not to scattering, but to the lack or receptors responsible for the perception of form and detail.

Short exposures. All the *O*s agree that at the very first moment of exposure form is present though it may not last long enough to be definitely identified. *M* often reported "at the very first there was a suspicion of form." *W* was most successful of all in reporting form after very brief exposures of stimulus. He succeeded in naming and drawing correctly the following figures which, however, appeared 'fuzzy' in various parts and lacked the distinct outline we are accustomed to find in good vision: triangle, half moon, cross, and rectangle. Probably most of the successes with forms in the long exposures were due to having seen them at the beginning.

What slight perception of form exists in the blind-spot furnishes conclusive evidence against the theories which assert that vision in the blind-spot is due to excitation of end-organs in the adjacent retina.

XIV. EXPERIMENTS DESIGNED TO TEST ALTERNATIVE THEORIES OF VISION IN THE BLIND-SPOT

Most of the theories concerned with vision in the blind-spot take no account of the possibility that the blind-spot may respond to direct stimulation, or at least disregard what is seen there under such conditions. Theorists have been engaged in explaining why there is no gap in the field of vision or why the optic disks may appear in contrasting colors to the rest of the field. Our experiments raise the problem as to how it is possible to see *with* the optic disk, a place where, it is assumed, receptors are lacking for the transformation of light into energy suitable for conduction along the nerves. Only two theories have been advanced in the literature to explain vision *with* the blind-spot: (1) that the light seen in the blind-spot is due to eye-movements causing the stimulus to stimulate contiguous retinal areas, advanced by Helmholtz to explain the fact that some of Donders' patients saw the image of a flame when thrown directly upon the optic disk; and (2) light directed to the optic disk scatters and falls upon neighboring parts of the retina, but it is assumed that what is seen is localized as coming from within the blind-spot. The authors of the contrast theories have not considered the possibility of direct stimulation of the blind-spot with resulting sensations of light and so their theories reduce to explanations in which the optic disk plays a wholly negative rôle, adjoining regions of the retina, or their central projections, being responsible for what is seen in the blind-spot. We shall therefore have to consider theories which *may* be advanced to explain our results, as well as theories already formulated since, with one exception, no thorough-going piece of work involving direct stimulation of the optic disk has yet been published.

It must be borne in mind that *evidence may be found to support any theory of vision in the blind-spot if contrary evidence is neglected,* as has been done all too often in the history of the problem. We have therefore selected those experimental tests which we believe to be crucial for all hypotheses which do not admit the possibility of direct response of the optic nerve fibers to light stimulation. Of the hundreds of experiments made and contemplated most seemed to be inconsequential so far as being definite tests of alternative theories. Hence only those tests will be given and discussed which seem to have a decisive bearing on the fundamental issues.

Series 1. The *Os* fixated on a lighted ground glass screen, illuminated from behind. The projection of the blind-spot (which we found by previous mapping) was then covered with a black cardboard. *There is no filling in of the blind-spot,* although light is stimulating the retina immediately contiguous to the optic disk, nor does the light take on the form of the blind-spot. In fact one observer (*G*) reports seeing clearly a dark disk in the place occupied by the blind-spot. Under these conditions no *O* reports the bright central core with surrounding haze or halo, so characteristic of vision in the blind-spot.

Series 2. Better than the above, but a modification of it. Instead of lighting the whole field around the blind-spot, we exposed a bright light in the projection of the blind-spot, and placed a piece of cardboard so that the only light present was that due to scattered light around the edges of the card. A faint haze was seen by all the *Os*, *but no O reported the bright central core.* In other words, when we have a gradient of brightness, greatest at the edges of the blind-spot and diminishing as we go away from it, the blind-spot is not filled in in the sense of the gradient as we should expect if the theories of scattering and irradiation were true. If we uncover the stimulus (so placed as to stimulate the blind-spot directly in the center) all *Os* immediately report the bright central core; and if we move the stimulus in the blind-spot the bright central core moves in a manner corresponding to the actual position of the stimulus.

Series 3. The above experiment more rigidly controlled. Mapping the blind-spot on screens at various distances, we obtained a series of projections increasing in size as we increased the distance. We then cut out the projection of the blind-spot in each screen, taking care that each map should be in alignment with the rest by special tests. We thus have a series of diaphragms to take care of scattered light. This procedure we believed was better than the use of a cone from eye to screen since the inside of a cone even though black would act as a multiple reflecting surface, giving scattered light rather than eliminating it. Under these conditions a weak stimulus was seen as a bright core with halo but if a small piece of black cardboard 1–½ the diameter of the blind-spot was placed in front of the light source, *the bright central core disappeared immediately,* although one half of the blind-spot was still stimulated by light. The core seen in the blind-spot corresponds to the brightest part of the source since covering that up caused the core to disappear. Even though the conditions postulated by the scattering theories were fulfilled in stimulating the periphery of the blind-spot no bright core was seen as we should expect if they are true. Nor did the light on the edges of the blind-spot take on the outline of the optic disk as we might have expected from the alternative theories. Since what has been said held true for the most intense stimuli (300 w.) used as well as for the weakest, our results cannot be explained as being due to insufficient stimulation. In using 300 w. light sources at 131 cm. from the *O* we actually obtained more light on the periphery of the blind-spot than would have been possible in the majority of our experiments carried on with weaker stimuli. Thus while we took special pains to fulfill the conditions of the irradiation and scattering theories the expected did not happen. We must conclude that the bright central core seen in the blind-spot under direct stimulation is due to the brightest part of the source, moving with it and disappearing when it is obliterated. Neighboring portions of the retina cannot be responsible for the bright core since no core is seen when the amount of scattered light is greatly increased but direct stimulation is absent.

Series 4. Still fulfilling conditions of the scattering and irradiation theories by varying *Series 3* as follows: we made rings of increasing brightness toward the boundaries of the blind-spot, thus actually having a gradient of brightness whose maximum was at the boundaries of the blind-spot. In some cases we even extended the rings into the blind-spot 2 cm. (see Figs. 24 and 27). Under these conditions, with light actually streaming in on all sides of the blind-spot and growing brighter as they approached the center, *no bright core appeared.* The completion in gradient of brightness in the blind-spot demanded by the scattering and irradiation theories did not take place even though, as here, optimal conditions were made for them. In fact we made conditions more in accordance with these theories than they were in the rest of our experiments, yet no bright central core appeared in the blind-spot, *one observer even perceiving a gap in the field where the rings ceased.* In some cases, even when the blind-spot was permeated by a haze from the rings extending into the blind-spot, no brighter core was present.

Series 5. Comparison of scattering in various parts of the retina and the blind-spot. It is generally conceded that the amount of scattered light is proportional to the intensity, and, we may add, to the length of fixation, increasing as the stimulus is fixated. Special experiments were made with *G*, *M* and *W* to determine whether the amount of visible scattering changed in different parts of the retina, especially near the borders of the blind-spot. While stimuli change in tint and chroma in passing from the fovea to the periphery there is very little difference in the amount of perceived scattered light as such since there is not enough to cause appreciable distortion of form. One *O* (*M*) found considerable difficulty in getting clear-cut forms in the periphery but reported no visible irradiation. Stimuli bordering the very edges of the blind-spot are seen as fairly sharp lines and do not diffuse into the blind-spot.

On the basis of the scattering theories we should have to assume that there is a sudden increase of scattering of light in the eye in one place—the region around the blind-spot. Moreover, the amount of scattering necessary would have to be so great to account for what is seen in the blind-spot that it passes belief, for normal vision with sharp contours could never then be possible. That scattering does not occur until we reach the inside of the blind-spot is proved by our experiments in which stimuli exposed on the edge of the blind-spot preserved their form. The diffuse halo in the blind-spot is due, then, not to scattering of light from the blind-spot to the adjoining regions and back again (irradiation theories require the transportation of light both away from and back to the optic disk) but to the nature of the optic disk itself which is composed of naked axones which are probably good refractors of light. The lack of receptors in the blind-spot would probably explain the lack of form perception.

Series 6. We know that stimuli falling within the blind-spot tend to spread or diffuse. The direction of spread can be reported upon by the *O* so that if we expose two stimuli within the blind-spot at different distances from the edges, we should expect, on the basis of alternative theories, the one nearest the edge to be the more intense, since the scattering is inversely proportional distance from sensitive elements. We exposed two stimuli 10 cm. from the nearest edge of the blind-spot and 20 cm. from each other to fulfill the distance condition of the scattering theories. Assuming that what is seen is due to stimulation of retinal areas adjoining the blind-spot by scattered light, we should expect the brightest parts to be near the edges of the blind-spot since the stimuli are nearer the edges than to each other. Under these conditions, meanings. Thus we expect the effect of scattering to be inversely proportional to the distance from the places affected by it and we can see if a stimulus near sensitive retinal elements is seen better in the blind-spot than one farther away. By a systematic variation of conditions we tested alternative theories of vision in the blind-spot and found that what we expected did not happen while in many cases what did happen is unexplainable if we accept these terms as we now understand them.

When, as in our experiments with a 2 mm. stimulus at 4 m. distance from the *O*, the image on the disk is only 1/200 the total area of the disk, the amount of scattered light reaching the neighboring retina must have been vanishingly small. Moreover, since the haze from this stimulus did not fill the blind-spot we should have to assume that a subliminal stimulus at the edges of the blind-spot can cause a supraliminal effect in the central region. This difficulty entails another—that of eccentric projection—for if surrounding rod-cone elements are responsible for what is seen in the blind-spot, why is not the stimulus localized on the edges as it is when we actually stimulate the boundaries of the blind-spot with an annulus of light? Furthermore, if the amount of scattering, diffusion and irradiation demanded by theory existed no clear vision would at all be possible. We showed that there was no sudden increase of scattering at the boundaries of the blind-spot and little *visible* scattering elsewhere. In fact Donders[52] was unable to detect any scattering around the edges of the blind-spot when he observed the optic disk through an ophthalmoscope when the image was 1/20 the diameter of the disk, while here we have worked with a ratio that was far greater, 1/200!

Another way of approaching the problem of scattering and blur circles is to calculate them, measure the amount present in actual observation, and then see if there is sufficient to account for our results. Volkmann[53] found that threads 0.0445 mm. were indistinguishable as two at such distance that the blur circles must have been 0.0035 mm. The image on the retina was 0.0019 mm. and hence the total image on the retina could have been not more than 0.0054 mm. Pope,[54] another investigator working with Helmholtz, found the irradiation circles to be 0.015 mm. *by experiment*. Listing[55] has given 0.022 mm. as the value of the diameter of a blur circle of a point 3 meters away from the eye, assuming the pupil to be 4 mm. wide. Using a somewhat different formula I have calculated the size of the blur circles from a point at 3-4 meters and find the value given by Listing to include the *third* maximum band.[56] If we take the first maximum to be 1, then the following bands have the following values:[57] 2 = 0.01745; 3 = 0.00415; 4 = 0.00165; 5 = 0.00078. It is very evident that the weakest stimulus seen in the blind-spot would not give rise to a noticeable sensation beyond the *third* band. Taking the value of 0.022 mm. as the limit of visibility of scattered light due to blur circles or diffraction bands and adding it to the size of the image of a 2 mm. stimulus at 4 meters, 0.005, we get the value of 0.027 mm. as the total effective image on the retina. Since the average blind-spot is 1.50 mm. we find we have a nearly 60-fold margin of safety before the edge of the blind-spot is reached. Here both theory and fact agree: the small amount of scattering is attested by observations in which it was reported that the phenomena seen do not extend to the boundaries of the blind-spot with the smallest and weakest stimuli used.

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[52] Donders, cited by Helmholtz, *op. cit.*, II, 27.
[53] A. W. Volkmann, cited by Helmholtz, *op. cit.*, I, 121 ff.
[54] B. A. Pope, Beiträge zur Optik des Auges, *Arch. f. Ophth.*, 9, 1863, 60-64.

That there is diffusion or scattering through the optic nerve head we do not deny. The nature of the phenomena observed in the blind-spot show it to be true. Since the diameter of the cones is given as from 0.002 to 0.004 mm. our 2 mm. stimulus at a distance of 4 meters would be large enough to affect only one and at the most two cones. Since a stimulus of this size does not appear as a point even in direct vision we should not expect it to in the blind-spot. But the amount of haze is so much greater in the blind-spot than elsewhere that one is inclined to ascribe it to the fact that end-organs are lacking and the optic nerve fibers act as refracting substances which tend to scatter the light within the optic nerve head.

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[55] Listing, cited by Helmholtz, *op. cit.*, I, 132.

[56] In my calculation the value of the longest visible wave-length was used because the diffraction bands are greater for the red end of the spectrum than for the violet. The formula used for the size of the diffraction bands, assuming a point source and that the pupil is a circular aperture, is as follows: $\frac{1}{2} X = d \frac{M\lambda \cdot 1.2}{W}$ where X is the total diameter of the diffraction band; d is 15.5 mm. or the distance from the nodal point of the eye to the retina; λ is the dominant wave-length of the source; M is the desired maximum (for 1st, M = 1, 2nd, M = 2, etc.); W is the width of the pupil and 1.2 is a constant used in the treatment of circular apertures. It will be seen that in order to get the diameter of the diffraction bands, i.e. the distance on either side of the center, we must double the right-hand member of the equation, hence the use of ½ X in the left-hand member.

[57] Taken from É. Verdet's *Leçons d'optique physique*, 1869, I, 306.

We have therefore three possible ways of explaining our results.

(A) What is seen is due to adjoining retinal elements. The following facts, in summary, argue against this conclusion:

(*1*) Some form perception exists in the blind-spot, vague, yet present.

(*2*) The least perceptible color seen in it is different from the faintest color seen on the edges.

(*3*) Two colors may be seen simultaneously within the blind-spot.

(*4*) When an annulus of color surrounds the blind-spot and another is exposed inside, neither affects the other, often a black space between is visible.

(*5*) Stimuli nearest the edges of the blind-spot are not the most effective in the blind-spot.

(*6*) Apparent movement may be seen in the blind-spot under conditions highly unfavorable to its perception outside.

(*7*) When the light seen in the blind-spot does not extend to the edges we should have to assume that subliminal stimuli outside the blind-spot can give rise to supraliminal effects inside.

(*8*) Characteristic peculiarities are found in the blind-spot which do not hold for other parts of the eye: peculiar changes in color with prolonged observation, rapid adaptation, and the like.

(*9*) Light does not take on the form of the blind-spot when only adjacent parts of the retina are illuminated while the blind-spot is kept dark.

(*10*) When the conditions demanded by the scattering and irradiation theories are fulfilled characteristic phenomena observed in the blind-spot, are lacking which are present during direct stimulation.

(B) The second hypothesis is that there may be end-organs in the optic nerve head which are responsible for the phenomena seen. The fact that several cores are seen when the stimulus has several centers of brightness or several stimuli are exposed simultaneously may argue in this direction. If such organs exist it is my opinion that they must be rudimentary and probably do not number over a few score at the most. It is to be hoped that histologists will investigate again this region of the eye since little is actually known concerning this point.

(C) The third hypothesis, that the optic nerve fibers are responding directly to light, fits in with a rapidly growing belief that all protoplasm is affected by light, that the phenomena of excitation and transmission of nerve impulses are electrical in nature, and should receive serious consideration. If it is remembered that definite receptors for smell, thermal, and the numerous cutaneous qualities have never been found the rod-cone hypothesis will not seem so sacrosanct.

But since we would expect the regions supplied by the nerve fibres to be seen if they respond to direct stimulation by light in accordance with the well-known principle of eccentric projection or constancy of place coefficients, it seems hard to believe that the fibres themselves are responsible for vision with the optic disk. We have pointed out above that this same objection applies to the hypothesis that adjacent retinal elements are responsible for what is observed in the blind-spot, because by the same token the stimulation should be referred to the places actually responsive to light and not to the optic disk. An objection which is equally fatal to the theories which do not allow the optic disk the functional rôle we have claimed for it does not help us in finding a positive explanation for vision there, even though it helps dispose of irradiation and allied hypotheses. We therefore find ourselves favoring the second of our hypotheses (B) which does provide the necessary mechanism in the optic disk without being open to the charge of violating the accepted principle of eccentric projection. If, however, stimulation along nerve trunks is not always referred to the regions supplied by the ends of the fibres, we should favor the (C) hypothesis.

AN EXPERIMENTAL STUDY OF FOUR CASES OF COLOR-BLINDNESS

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Most investigations which have been made with color-blind subjects have been largely of a clinical nature, for the purpose either of classifying the type or degree of color defect or to standardize color tests. The present study was undertaken with the view of submitting color-blind subjects to a group of standard visual experiments and comparing the results with those of normal subjects.[1]

Subjects. Four color-blind *S*s (*A*, *B*, *C*, and *D*) and two normal *S*s (*E* and *F*) were used. The color-blind *S*s were students of General Psychology who reported in class or to their respective instructors that they were color-blind. Since the color condition of *A* and *B* is quite unusual, a detailed account of preliminary tests used with them is given.

Preliminary tests with A. *A* claimed to be totally color-blind. One of his grandfathers is said to have been color-blind, and a younger brother is said to have the same sort of color defect as *A*. Since early childhood *A* has been handicapped by poor vision and has worn very thick glasses. He says that he can see better in artificial light and prefers to do most of his reading at night under artificial light. He has a peculiar kind of nystagmus, the eyes twitching and moving together to the right and a little downward on a line at an angle of approximately 60° from the horizontal. When in a reflective state, the eyes are noticed to be in this turned position, which seems to indicate that it is their natural resting position. When he is looking directly at an object the pupils are turned upwards and slightly outwards, which leads one to conclude that the point of most distinct vision lies somewhat above the fovea and towards the outside of the eye.

A's visual acuity as tested by the Lowell test type is 1/10 for each eye, the left eye being a little stronger, since the test type appears clearer to that eye. This defect is not corrected by the glasses he wears. Both eyes were found to be slightly astigmatic. The field of distinct vision, which was plotted by a modified form of campimeter, is much smaller than normal. Foveal blindness could not be detected, though there is indistinctness in that area.

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[1] The experimental work was carried on in the Psychological Laboratory of the University of Pittsburgh, with the assistance of G. J. and A. M. Snyder.

Five color tests were used and the results are recorded in some detail. All this work, as well as the experimental work described later, was done in a dark room under an artificial daylight lamp with simplex multiplex reflector.

(1) Sorting of colored splints. For this test a set of Milton-Bradley Co.'s kindergarten splints was used. The splints were mixed up in a heap on the table and *A* was directed to arrange them in piles according to color. *A* spent a long time at the task, and when he had finished had 11 piles, each of which he was then asked to match with a gray in the Hering booklet of 50 shaded grays. His grouping was as follows:

Group	Kind of splints	Gray to match
I	4 yellow	7
II	5 yellow	8
III	1 blue	10
IV	1 orange	17
V	7 blue, 3 green, 1 orange	20
VI	3 orange, 2 green, 1 blue	25
VII	4 green, 1 orange	30
VIII	5 orange, 1 green, 1 blue, 1 purple	33
IX	5 purple, 1 green	34
X	5 purple	45
XI	11 red	50

(2) Holmgren wool test. A set of Holmgren worsteds consisting of the first 40 skeins of the original test was used with the standard directions. The following matches were made. To match test skein A (green), *A* picked out 14 (green), 28 (rose), and confusion colors 5 (pinkish gray) and 29 (light blue). To match skein B (rose), he picked out 28, 30 (rose), and confusion colors 7 (brownish gray), 9 (gray) and 13 (dull yellow). To match skein C (red), he picked out 32, 34, 36, 38, 40 (red) and confusion color 37 (reddish brown). Red to *A* appears very dark, almost black, so that in matching the red skein he picked out the darkest ones.

(3) Color equation test. A color-mixer with two sizes of paper disks was used. The matches made were:

Outside disk	Inside disk
Blue	Yellow (125°) and Black (235°)
Orange-red	Green (16°) and Black (344°)
Orange-red (238½°) and White (121½°)	Green (197½°) and Black (162½°)

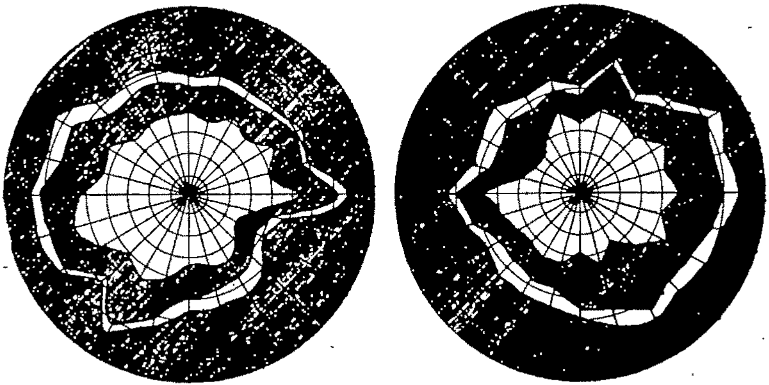
(4) Nagel card test. This test was very difficult for *A* and his responses were little more than guesses. None of the spots seemed to him dark enough for red. The brown spots on card 12 appeared closest to what he called red. To the questions which involved picking out cards with one color only, *A* replied that there were no cards containing just one color.

(5) Sorting of color cards and gray cards. A set of 164 cards (1 × 2½ in.) was used. These consisted of the 90 Bradley educational colored papers and 10 grays of the Milton-Bradley sample booklet, 48 of the 50 Hering grays, 14 miscellaneous colors and 2 velvet blacks. The cards were scattered promiscuously on a sheet of white cardboard and the *S* was directed to put together the cards that looked *exactly* alike. *A* arranged the cards into 21 groups

of such promiscuous grouping of colors that it was evident that in this test as in the preceding ones his matches were made on the basis of differences in brightness only.

A is, it seems safe to conclude from the results of these tests, totally color-blind.

Preliminary tests with B. *B* reported to his instructor that he was totally color-blind. He suffers considerably from eye-strain which necessitates constant wearing of glasses. No peculiarities of eye-movement can be detected, nor is there any evidence of foveal blindness, though his visual field is very small. His pupils are noticeably small. Visual acuity is 1/10 for the left eye and 3/20 for the right eye, without glasses, and 5/20 for left eye and 8/20 for right eye with glasses. There is no evidence of astigmatism. In the perimeter test it was discovered that *B* had a nearly circular band of blindness in each eye. The shaded areas of the accompanying diagram, Fig. 1, are areas of total blindness.



RIGHT EYE LEFT EYE

FIG. 1. SHOWING B's FIELD OF VISION

The shaded areas are areas of total blindness

B was put through the same color tests as *A*. From the beginning it was noticed that B's discrimination of brightness was poor and that he reacted differently towards blue than towards any other color.

(*1*) *Sorting of colored splints.* *B* was able to make but 2 piles: one containing yellow and orange and the other containing the 4 remaining colors.

(*2*) *Holmgren wool test.* The large set of worsteds was used with 2 additional test skeins, a blue-green (3) and a yellow (Y). In making his matches *B* mixed up the various colors without regard to hue with one exception, the group picked out to match the blue-green was more uniform. Most of the colors placed in this group were bluish: 22, 24, 30, 72, 74, 76, 78 (blue); 28, 70, 84, 86, 88, 90, 92 (violet); 21, 25, 79 (rose); 7, 11, 41, 57, 77 (green).

(3) *Color equation test.* Since *B*'s discrimination of brightness is very poor, the proportion of black or white in a mixture cannot be considered as definite. The equations he made were:

Outside disk	Inside disk
Orange-red (270°) and White (90°)	Green (356°) and Black (4°)
Blue	Yellow (61°) and Black (299°)
Black (263°) and White (97°)	Green
Black (213°) and White (147°)	Yellow

For the second equation, *B* said, "They are of the same brightness, but look like two different colors." He was unable to name the colors.

(4) *Nagel card test.* The spots on the Nagel cards all looked the same to *B*, due to poor brightness discrimination as well as to color-blindness.

(5) *Sorting of color cards and gray cards.* *B* made a very promiscuous grouping of these cards into 10 groups. It was noted that whereas all other groups contained a mixture of various colors, all those placed in Group III contained some blue. The cards in this group were picked out from the others very quickly.

(6) *Special test for blue.* In order to determine whether or not *B* could see blue, a special test was devised. A set of cards consisting of the 50 Hering grays and the 18 standard colors of the Bradley series was scattered on a sheet of white cardboard. *B* was asked to put into groups those that looked alike. The following 5 groups were made, ranging from lightest to darkest.

Group I: Grays 1, 2, 3, 4, 5, 6, 8; orange-red; red-orange; orange; yellow-orange; orange-yellow; yellow; green-yellow; yellow-green. These he called white.

Group II: Green-blue; blue; violet-blue; blue-violet; violet.

Group III: Grays 7, 9, 10, 11, 12, 13, 14, 15, 19, 22, 23, 24, 28; blue-green; red-violet.

Group IV: Grays 16, 17, 18, 20, 21, 25, 26, 27, 29, 30, 32, 33, 34, 35, 36, 37, 38, 39, 40; violet-red; red; green.

Group V: Grays 31, 41, 42, 43, 44, 45, 46, 47, 48, 49, 50. These he called black.

It can be noticed that all the cards of Group II are bluish in hue. *B* said this group was of the same brightness as the third group and yet the two groups could not be classified together, for there was something different which characterized Group II. He was not sure whether the cards of Group II were colored or not and when asked to guess their color was unwilling to do so, but finally said, "They may be blue—or green—or red. I don't know."

B was told that these cards which he called "different" were blues. Then all the cards of all 5 groups were mixed up again. *B* was directed, "Pick out all the blues." Without hesitation, *B* went through the 68 cards and picked out the same five bluish cards.

B's condition can be described by saying that he is red-green blind. He is able to distinguish blue and, while unable to distinguish yellow, his difficulty here may be due to the fact that it is so light as to be confused with white.

C's visual acuity for both eyes is 1/10 without glasses and 7/10 with glasses. Both eyes are astigmatic. The color tests indicate that he is red-green blind. His chief difficulty seems to be with light red, light green and gray, which he is unable to tell apart. There is also some confusion between red and brown. He seems uncertain as to what gray is and seldom uses the term. Usually grays are called either red or green by him.

D's visual acuity without glasses is 5/10 for both eyes. With glasses it is 6/10 for the left eye and 8/10 for the right eye. He is not astigmatic. He wears glasses only when doing close work. An operation on the left eye several years ago left this eye weaker than the right eye. The color tests indicate red-green blindness with especial difficulty in distinguishing green, particularly dark green which he confuses with gray. His maternal grandfather was color-blind.

E and *F*, instructors in the Department of Psychology, were used as control *S*s. *E*'s visual acuity is 1.5 for the left eye and 5/10 for the right eye without glasses, and 1.5 for left eye and 1.2 for right eye with glasses. *F*'s acuity is 5/10 for left eye and 6/10 for right eye without glasses, and 1.5 for left eye and 8/10 for right eye with glasses. Both *E* and *F* are slightly astigmatic, which condition is not corrected to any great degree by the glasses they are wearing. Both passed all the color tests used.

I. BRIGHTNESS DISCRIMINATION

The object of this experiment was to determine difference limens for brightness.

Procedure. Use was made of a color-mixer, one black and one white Bradley disk each 19½ cm. in diam., one black and one white Bradley disk each 11½ cm. in diam. and a screen to conceal the mixer while adjustments were being made and until the mixer was going at the proper speed. The Mazda lamp was placed 39 cm. from the faces of the disks to properly illuminate them while they were being exposed to *S*. To avoid the light reflection that so often interferes with observing a flat surface, it was found best to have the *S*s observe the rotating disks from an angle of 50° with the plane of the surface of the disks. The *S*s made their observations from a distance of 2½ m.

The method of constant stimulus differences was used.[2] The two sets of disks were placed on the mixer together. The inside disks were arranged so as to expose 20° of white, and were kept this way as a standard while variations in the proprtions of white and black were made in the outside disks. Then the standard of 60° white was used, and later one of 180° white. After adjustment was made the disks were set rotating. The *S* was directed, "You are to tell me whether the outside gray is darker, the same as, or lighter than the inside gray." 'Ready' and 'Now' signals were given. At the signal, 'Now,' the screen was removed.

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[2] E. B. Titchener, *Experimental Psychology, Quantitative, Student's Manual*, 1905, 92 ff.

A preliminary series was taken for each position to determine the limits to be used. Five comparison stimuli were chosen at equal intervals around each standard. These 5 included the standard and at least one above and one below, the standard not necessarily being the middle value. The main part of the experiment consisted in going through 25 series of judgments for each of the 3 positions, a series consisting of 5 judgments—one for each comparison stimulus. The order in which these were given was determined entirely by chance.

It was necessary to use a different method with *B*. His judgments varied from day to day to such an extent that comparison stimuli chosen one day would not hold the next day. Twice new values were chosen and the experiment was begun again, *B*, however, not being aware of this. It was necessary finally to use the method of limits. A reading was taken at a place so far below the standard that the judgment would probably be 'darker' (if such a point could be obtained). Readings were then taken at regular intervals until the judgment 'same' was given. Then a set of readings was taken, beginning at a place where the judgment would probably be 'same' and continuing until the judgment 'darker' was given. The procedure was repeated 9 times. The same method was used to determine the upper limits. Lower limits could not be reached for 20° and 60°, nor an upper limit for 180°. *B*'s results, therefore, cannot be directly compared with the others. It can only be said that he falls far lower in brightness discrimination.

In computing the results, the percentages of judgments 'darker' and 'lighter' were found, and the difference limens obtained by means of the phi-gamma hypothesis, using Urban's tables[3] and Rich's checking tables.[4] The upper limen for 180° for *F* had to be computed by means of Spearman's method of Right and Wrong Cases,[5] since there was but one comparison stimulus for which a percentage different from 0 or 100 was obtained.

Results. Table I shows the lower, upper, and average limens (L) for each position, together with their respective measures of precision (h). Comparison of *S*s can be made on the basis of the average of the relative difference limens for each position, the relative difference limen (RDL) being the average difference limen divided by the standard. Thus at 20°, the RDL is the average limen divided by 20.

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[3] E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280-293.

[4] G. J. Rich, A checking table for the method of constant stimuli, this JOURNAL, 29, 1918, 120-121.

[5] C. Spearman, The method of 'Right and Wrong Cases,' (constant stimuli) without Gauss's formulae, *Brit. J. Psychol.*, 2, 1908, 227-242.

mixer B were changed until the gray seemed to be of the same brightness as the gray of the ring. Five comparison stimuli were chosen by means of a preliminary experiment. The method of procedure and of computing results was the same as in the first experiment, except that only 20 series of judgments were taken.

Results. Table III gives the results obtained. The limens here are given in number of degrees instead of as differences from a standard. Since there is no standard from which to work and since the arbitrary standard chosen in computation was different for each *S*, comparison was made on the basis of the points of subjective equality. The point of subjective equality is defined by Urban as the point at which the probability of one of two judgments equals the probability of the other judgment. In this experiment it is the point where the probability of the judgment 'darker' equals the probability of the judgment 'lighter.' This point is given by the value $\frac{h_u L_u + h_l L_l}{h_u + h_l}$.

TABLE III

SHOWING THE EFFECT OF CONTRAST ON THE LS, ON THE INTERVALS OF UNCERTAINTY, AND ON THE POINTS OF SUBJECTIVE EQUALITY

	S	L		h		Interval of Uncertainty	Point of Subjective Equality
		Lower	Upper	Lower	Upper		
White Background	*A*	33.38	45.68	0.110	0.081	12.30	38.57
	B		129.55		0.125		
	C	26.68	46.79	0.125	0.046	20.11	32.11
	D	14.28	16.13	0.174	0.148	1.85	15.13
	E	20.49	29.72	0.046	0.069	9.23	26.04
	F	8.68	26.45	0.047	0.099	17.77	20.17
Black Background	*A*	82.29	117.69	0.096	0.023	35.40	89.29
	B	24.06		0.064			
	C	154.98	169.64	0.051	0.029	14.66	166.68
	D	240.97	240.97	0.025	0.025		240.97
	E	155.89	183.56	0.042	0.031	27.67	167.68
	F	189.01	251.31	0.012	0.025	64.30	230.58

Table IV gives the same results in another form. The *S*s are arranged in order of the contrast effect obtained, the contrast effect being considered the difference between the standard (*i.e.* the 80° white in the gray ring) and the point of subjective equality.

It will be noticed that the order of *S*s here is the same as in Table II, which seems to indicate that there is some relation between ability to discriminate brightness and ability to get a decided contrast effect.

B's results, again, cannot be compared with those of the others, since neither the lower limen with the white background nor the upper limen with the black background could be reached. The interval of uncertainty with the white background lies between 129.55° white and some point darker than the black used. With the black background the interval of uncertainty lies between 24.06° white and a point lighter than the white used. *B* was able to see the difference between the darkest and the lightest points used, but with a white background no point could be reached that seemed to him darker than the gray of the ring, while with a black background no point could be reached that seemed to him to be lighter than the gray of the ring. All that can be said in reference to *B* is that he does obtain a contrast effect, though a direct measure of it could not be obtained.

TABLE IV

SHOWING THE CONTRAST EFFECT FOR THE DIFFERENT *S*s

S	White Background	Black Background
D	64.87	160.97
F	59.83	150.58
E	53.96	87.68
C	47.89	86.68
A	41.43	9.29
B	——	——

An examination of Table IV shows that the order is the same for both the white and black backgrounds, but whereas with the white background each *S* differs from the one following by practically 6°, with the black background the differences are very great. In the latter case, *D* and *F* stand very close together at 160.97° and 150.58° respectively. Then there is a large interval between *F* and *E*. The contrast effects of *F* and *C* are very close, differing by but 1°. There is another big difference between *C* and *A*. *A*'s contrast effect is but 9.29°, his gray being made but 9.29° lighter by the black background. With the white background, though he obtained the least contrast effect of all the *S*s, it was between 4 and 5 times greater than this—41.43°.

III. RELATIVE BRIGHTNESS OF COLORS

Here the object was to determine the brightness values of the four colors (red, yellow, green, and blue) and to find the comparative order of brightness of these colors for each *S*.

Procedure. One color-mixer was used with colored disks (19½ cm. in diam.), the colors being the standard red, yellow, green, and blue of the Bradley series, and a set of adjustable black and white Bradley disks (11½ cm. in diam.). The *S*s observed as in the first two experiments. The directions given were, "You are to tell me whether the color is darker, lighter, or of the same brightness as the gray." Twenty series of judgments were made for each color.

Results. The results are shown in Table V. The upper limens for green and yellow could not be found for *B*. From a few judgments which he gave as 'lighter,' the upper limen for red was found to be theoretically 47.02° from 350°. The data that can be obtained for *B* by means of the method used are not as reliable as those for the other *S*s. For instance, in the case of red and blue, the upper and lower measures of precision (h) differ so greatly that the points of subjective equality are quite different when computed by the formula than when taken as the mid-point of the interval of uncertainty.

TABLE V

SHOWING THE RELATIVE BRIGHTNESS OF THE VARIOUS COLORS FOR THE DIFFERENT *S*s

Color	*S*	L		h		Interval of Uncertainty	Point of Subjective Equality
		Lower	Upper	Lower	Upper		
Red	*A*	(6.50)	(5.68)	.332	.316	0.82	(6.10)
	B	10.00	397.02	.181	.012	287.02	35.54
	C	21.57	31.38	.173	.134	9.81	25.83
	D	43.77	49.59	.145	.214	5.82	47.24
	E	34.65	44.77	.119	.103	10.12	39.36
	F	32.81	45.10	.075	.077	12.29	39.02
Green	*A*	146.96	150.88	.071	.064	3.92	148.82
	B	20.17		.108			
	C	85.88	105.27	.059	.048	19.39	94.51
	D	106.09	113.36	.168	.099	7.27	108.75
	E	113.95	123.48	.091	.137	9.53	119.66
	F	93.79	107.06	.126	.066	13.27	98.36
Blue	*A*	83.47	88.55	.123	.137	5.08	86.14
	B	13.87	184.60	.055	.008	170.73	35.31
	C	34.88	44.03	.232	.139	9.15	38.31
	D	22.22	23.84	.214	.235	1.62	23.07
	E	30.44	40.63	.050	.081	10.19	36.74
	F	30.45	41.89	.134	.122	11.44	35.91
Yellow	*A*	234.01	237.31	.080	.086	3.30	235.71
	B	27.46		.035			
	C	76.41	132.15	.021	.011	55.74	96.45
	D	331.18	350.96	.043	.039	19.78	340.57
	E	315.47	343.43	.074	.033	27.96	314.99
	F	305.00	332.51	.041	.036	27.51	317.87

Red is such a dark color for *A* that it was darker than the black used. His readings for red were taken, therefore, in comparison with velvet black.

It is to be noticed that *A*'s red is very much darker than the average. His yellow is 32.66° darker than the average and 80.72° darker than the normal *S*'s yellow. His green, on the other hand, is lighter than the average by 43.5°. These results are typical of rod vision, and are, therefore, what one would expect in a case of total color-blindness.

The two normal *S*s stand very close together in their judgments of brightness for all colors. *C* and *D* show the greatest differences. *C* sees red as darker than the normal *S*s, *D* as lighter. *D*'s green is about the same as that of the normal *S*s, *C*'s is darker. *D*'s blue is darker than the normal, *C*'s is lighter. *C*'s yellow is quite dark, *D*'s is lighter than that of the other *S*s.

IV. NEGATIVE AFTER-IMAGES

The purpose of this experiment was to determine how the color-blind *S*s differed from the normal *S*s in ability to obtain negative after-images,

Procedure. The after-images were projected on a neutral gray background. The cards (¾ in. × 1 in.) fixated consisted of 10 Hering grays (1, 5, 10, 15, 20, 30, 35, 40, 45, 50) and 8 Bradley colors (red, green, yellow, blue, violet-red shade 1, yellow-orange, blue-green, and blue-violet shade 2). The last four colors were chosen because they were found to be the nearest matches for the after-images of the first four.

The cards were arranged about the gray background. After getting an after-image for one of the cards (which was placed in the center), *S* pointed out from the other cards the one that most closely resembled his after-image. In the color series, if none of the cards of the series matched the after-image, *S* was allowed to select the nearest match from the colors in the Bradley sample booklet.

Results. The results are given in Table VI. A blank space in the table means that an after-image for this card could not be obtained. A dash between two numbers means that the after-image was between those two. The numbers refer to the grays and the letters to the colors. A boldface number or letter indicates that the after-image was hazy and indistinct. An asterisk (*) indicates that the after-image was very clear, appeared before the card was removed, and was persistent.

A had considerable difficulty in getting after-images. He said that he had never been able to obtain negative after-images, but could get positive after-images very easily when he closed his eyes. The first time he tried this experiment, he was unable to get any negative images. The next day black and white backgrounds were used. Number 50 of the gray series (black) was first tried on a white background. After a fixation time of 75 sec., he saw the negative after-image, *i.e.* a whiter patch on the white background. Number 1 of the gray series (white) was next tried on the black background. The after-image was like 50, the background appearing lighter, like gray 30. After this practice, the regular series was begun, but he could obtain after-images for only the lightest and darkest cards.

TABLE VI

SHOWING THE QUALITY OF THE NEGATIVE AFTER-IMAGES INDUCED BY THE VARIOUS STIMULI-CARDS

A fixation-time of 90 sec. was used for *A* and *B*, and of 60 sec. for *C*, *D*, *E*, and *F*

Quality Stimulus-Card	Subject *A* After-image	*B* After-image	*C* After-image	*D* After-image	*E* After-image	*F* After-image
Gray 1	40	50	35	40	40	40
Gray 5			30	35	20-30	30-35
Gray 10			20	35-40	20	20
Gray 15			20		15-20	15-20
Gray 20						
Gray 30					15-20	15
Gray 35			**10**	10	10-15	15
Gray 40			5	5-10	10	10
Gray 45	5	1†	5	5	10	5
Gray 50	10	1	5	1-5	5	1-5
Red	5-BG	B or 40-45	BGt1	**BG-BGs1**	BG-BGs1	BG-BGs1
Yellow		B*	BVs2	VB*	BVs1*	BVs1*
Green	Y or 1-5		VR	**VRs1**	RV	RV
Blue		R*	YO	YO*	OYs1*	OYs1-OYs2*
Violet-red s1	B or 10-15	B*		Gt1	G	G
Yellow-orange				B*	GBs1*	GBs1*
Blue-green	(1) Y (2) 20			**R**	VR-VRs1*	VR
Blue-violet s2		YO*		Yt1*	YGs1	YGs1

*After-image was clear, appeared before card was removed, was persistent.
†Fixation-time 150 sec.
Boldface indicates a hazy and indistinct after-image.

An interesting thing to note is that *C* had considerable difficulty to obtain an after-image for green, while *D* had most difficulty with red. These after-images could not be obtained until a second trial. Though *D* had most difficulty with red, when he did succeed in getting the after-image it was like that of the normal *S*s, between blue-green and blue-green shade 1, while *C*'s after-image was lighter, blue-green tint 1. *D*'s, however, was very indistinct. *D*'s after-image for green, while rather hazy, was darker than that of the normal *S*s. *C*'s was a little different, violet-red in place of red-violet. *D*'s after-image for blue-green was a red which was hazy and indistinct. Due to lack of time, *C* did not go through the last four.

SUMMARY

A shows every indication of being totally color-blind. Evidences of rod vision are: (1) his point of distinct vison is not at the fovea; (2) his vision improves at twilight; and (3) his red and yellow are darker while his blue and green are lighter than normal, which is typical of twilight vision. His brightness discrimination is below the average and the effect of contrast much less. Negative after-images are rarely obtained.

B's color field is restricted to blue and in each eye there is a circular band of blindness. His brightness discrimination is very poor, as he can distinguish approximately but 10 shades of gray. He can obtain after-images for the lightest and the darkest grays or colors only. His results from the other experiments cannot be compared with those of the other *S*s because of his inability to reach certain limits.

C, who was found by these experiments to be partially red-green blind, is below normal in brightness discrimination, brightness contrast, and ability to obtain negative after-images. He sees all colors tried in section III darker than the normal except blue.

D, the second partially red-green blind *S*, is normal in brightness discrimination and ability to obtain contrast. He is unable, however, to obtain as distinct after-images as the normal, especially for red and green. His red and yellow are a little lighter than those of the normal *S*s, his blue darker.

The animal was responding not to the tone but to the food which, in all the experiments involving discrimination, was associated with every response, and even in the absence of the tone the animal still responded to some stimulus in the general situation by going toward the place where it usually found food. It is probably not a cause for wonder that so relatively weak a stimulus as a tone failed to assume a significant place in the situation within which the animals were reacting.

The successful formation of the habit when noises were substituted for tones can perhaps be explained in two ways. In the first place a noise is obviously a 'larger' stimulus than a tone. It is a grosser, more compelling stimulus and makes up a relatively larger part of a total situation than does a tone, and therefore stands a greater chance of becoming a significant part of the experimental situation. A remark by Raymond Dodge furnishes a second possible explanation. The articular mechanism of both vertebrates and invertebrates seems to be an extremely sensitive receptor mechanism for the apprehension of vibratory disturbances in supporting structures. Hence it is not improbable that the disturbances which were set up by the buzzers, hand-claps, and taps on a wooden stimulus board were transmitted through the apparatus to the animals.

It is the writer's opinion that in general the discrimination maze-method is useless for problems involving tonal discrimination because it does not offer adequate possibility of control. It is impossible to eliminate from the situation other auditory stimuli, visual stimuli, and the stimuli furnished by the animal's own body due to hunger and other unpredictable conditions which may exist in the organism. The differentiating response, in any method, must involve the entire animal within the total situation, and the response will, in all cases, be a function of the total situation as a 'stimulus.' It seems to be too much to expect the animal to ignore the preponderance of extraneous stimuli for so relatively small a part of the situation as a pure tone. Until the technique of animal experimentation is developed to such a degree that we can instruct the animal as to just what stimulus he is to attend to, as we do in the case of human subjects, the maze-method will furnish no results significant for the present purpose.

As positive evidence of the ability of the higher vertebrates to apprehend tones, we have the findings of several Russian experimenters who worked with

Pavlov's method of using the salivary reflex as an indicator of conditioning. Seliony, Makovsky, Boormakin and others, in several researches demonstrated that the dog possesses a high differential sensitivity for tones, and an upper limen in the neighborhood of 100,000 s.v./s.[6] But the results of many of these investigators are vitiated by the fact that the investigators remained in the room with the animal during the experimental period, and might have offered secondary cues which served to guide the reactions of the animals.

The results of an experiment by Anrep, however, seem to stand unambiguously as positive evidence of the dog's ability to apprehend pure tones.[7] He used Pavlov's method and exercised every possible control to prevent the influence of secondary factors upon the reactions of his animals. The latter were placed in a sound-proof room during the experimental period and the *E* made his observations from an adjoining room. He obtained clear evidence of discrimination between the conditioning tone of 637.5 and another tone of 680 d.v./s.

The salivary reflex method has been criticized on the ground that an auditory stimulus might be strong enough to elicit a salivary reflex but too weak to influence the voluntary muscular system. Anrep states that he many times observed that the dogs upon hearing the active sound (the conditioning tone) attempted to reach the food plate by bringing into play the entire muscular system. The sounds of other tones left them quiet or evoked a negative motor reaction and the dogs turned their heads away from the food receptacle on hearing the sound.[8] Anrep concludes that "dogs possess a full pitch discrimination power and that if a suitable method is used it can be observed on muscular and salivary reflexes equally well."

II

The method utilized in the present experiment involved conditioning guinea pigs so that they would invariably respond by a change in the respiratory rhythm when tones of a particular pitch were sounded. If such a response could be established in the absence of secondary cues, it would seem sensible to assume that pure tones are effective as stimuli in the environment of the guinea pig. The method afforded opportunity for the collection of permanent objective records of responses and the relations of the responses to significant stimuli; a desirable condition in any experiment with animals. Furthermore, very thorough control of the experimental situation was possible and extraneous stimulation by sound or light and such generalized factors as hunger were easily eliminated.

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[6] Reference to these three writers is to be found in Anrep's work on pitch discrimination in the dog, *J. Physiol.*, 53, 1920, 367-385. The work was done as a doctoral dissertation at Petrograd in 1907, 1908, and 1909.

[7] G. V. Anrep, *op. cit.*

[8] G. V. Anrep, *op. cit.*, 385.

The apparatus used in the present experiment comprised the following.

(1) *The tone producing apparatus*, consisting of two General Radio, Type 213, oscillators,[9] with frequencies of respectively 1000 and 600 cycles; a Bosch small size loud-speaker, mounted in a sound-proof box; a four-cell Edison storage battery (5.2 volts); and a specially designed three-contact key.

(2) *The electrical stimulating apparatus*, consisting of a Harvard Apparatus Company inductorium; a two-contact key; a six-cell Edison storage battery (7.8 volts); and two simple aluminum electrodes.

(3) *The recording apparatus*, consisting of a tambour-type pneumograph and tambour for recording the respiration of the animal and two electrical markers for recording the time and duration of the shock and tone. A time marker furnishing one second intervals was used and all the records were taken on a smoked-drum kymograph.

The first contact of the three-contact key controlled the primary of the oscillator; the second, the electrical marker for recording the time and duration of the tone; and the third was connected in series with the key controlling the shock, so that a break of the three-contact key would insure simultaneous cessation of both shock and tone. This series-connection of the two keys served further to prevent the animal's being shocked without first hearing the tone.

The first contact of the two-contact key controlled the primary of the inductorium, and the second the electrical marker for recording the time and duration of the shock. A switch was placed in the secondary of the inductorium and this circuit was broken when the test tone (*i.e.* tone without shock) was presented, to prevent any possibility of the animal's being shocked by a current set up in the secondary of the inductorium by the field of the oscillator coils. An ordinary electric buzzer in the primary circuit of the inductorium served as a circuit breaker. This buzzer was placed in an adjoining room to eliminate its noise from the experimental situation. Throughout the experiment extreme care was exercised to insure silence during the experimental period. The keys were padded and the contacts were of the wiping type, which insured a silently working mechanism. The sound-proof box was raised from the surface of the table upon which it rested by heavy felt pads and all other apparatus rested on another table separated from the first one by several feet. The keys were fastened to a heavy board (18 × 24 × 1 in.) which was separated from the top of the table by a piece of felt, 1 in. thick, and approximately the size of the board. A distance of 3 ft. separated the inductorium from all other electrical apparatus. The electrical markers, padded with rubber, were adjusted so that they were practically noiseless in their action. The greater part of the experimentation was carried on at night to escape the usual disturbances that occur in a laboratory during the day.

III

Four animals ranging in age from 8–12 mo., were used in the experiment. An experimental animal, held in a suitable holder, was placed in the sound-proof box which held the loud speaker. A distance of 24 in. separated the animal from the source of the tone. The conditioning tone had a frequency of 600 d.v./s. Its intensity was the lowest obtainable from the oscillator.[10]

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[9] H. M. Halverson, Description of the audio-oscillator, this JOURNAL, 38, 1927, 294-295.

As a first step in the experiment, each animal was tested to determine whether presentation of the tone would be followed by a change in the recorded breathing movements. Fifty tones were presented to each animal with haphazard spacing ranging from 10 sec. to 5 min. There was no record of change in the breathing of any of the animals that could in any sense be correlated with the presentation of the tones.

With the preliminary testing concluded, the conditioning process was begun. Each experimental period lasted 60 min. and there were 4 of these periods weekly for each animal, until the conditioned response was established. The sound-proof box was aired every 15 min. during the 60-min. periods by opening the door to the animal compartment and allowing a current of air from a small electric fan to play into it for 1 min. After being placed in the apparatus, the animal was subjected to tones of short duration (lasting approximately through 6 respiration cycles) and a shock was administered which lasted approximately through the last sixth of the tone. Both tone and shock were terminated simultaneously. The shock was not strong enough to disturb the animal greatly, and resulted in slight bodily movement only, caused by flexing of the rear legs to which the electrodes were fastened. The time between the tone-shock presentations varied from 3 sec. to 5 min. throughout the experimental period. At the beginning of the experiment no change in the breathing occurred when the tone was sounded; but, as was to be expected, a gross change was observable when the shock was administered, which was recorded in the breathing-curve as a breathing-cycle of great amplitude. Whether this wide excursion of the writing lever was caused by general bodily activity, due to peripheral stimulation, or to a deep inspiration, it is impossible to say. However, this point is not significant, since the response was uniform in nature and unambiguously correlated with the occurrence of the shock. Occasionally the shock was omitted, and the tone presented alone. Omission of the shock ranged from every twenty-fifth to every fiftieth tone-shock presentation.

Under normal experimental conditions the breathing of the animals was very irregular. There was wide variation in the amplitude of the respiration cycles as recorded in the breathing curve, and this irregularity of the breathing curve served as a background for one of the clearest indications of conditioning. At about the two hundred and fiftieth tone-shock presentation, a peculiar smoothing out of the breathing curve occurred. The respiration cycles correlated with the duration of the tone and preceding the shock were equal in amplitude, whereas, in that part of the breathing curve not coincidental with tones, wide and constant variation of amplitude characterized the curve. After this characteristic response had once appeared, it occurred regularly with the sounding of the tone, both in the tone-shock presentation and when the tones were sounded without the shock. At the termination of the tone the irregular breathing again set in. This response to the sounding of the tones, which was possibly a sort of conditioned inhibition, will be referred to hereafter as the 'lullaby effect.' On several occasions the tone was sounded for longer periods, varying from 12 to 20 respirations. The 'lullaby effect' continued through the longer periods just as clearly and unambiguously as it did through the shorter periods.

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[10] The type 213 General Radio oscillator is equipped with 4 terminals from the secondary coil. The first one is the common lead and the other 3 vary in energy output, giving 3 discrete intensities.

Although the 'lullaby effect' was regarded as clear evidence that the animals were responding to the tones, it seemed that in time some response should occur in place of the shock when it was left out of the presentation. This expectation was fulfilled, and after about the five hundredth tone-shock presentation what may be considered the true conditioned response appeared. A great increase of the amplitude of the breathing cycle correlated with the end of the tone, or slightly later than the occurrence of the shock in the tone-shock presentation, was observable in all the test-tone presentations (*i.e.* the tone presentations without the shock).

It was felt that the beginning and end of a tone might have furnished a possible secondary cue to which the animals could have been responding. When the key which controlled the oscillator was closed the tone came on suddenly, and when the key was opened it ended suddenly. There was a click-like quality to this sudden inception and ending of the tonal stimulus. In order to determine the influence of this aspect of the stimulus on the responses, the tone characteristic was eliminated and the clicks presented alone. To eliminate the tone the vibrating surface or diaphragm of the loud speaker was loaded with a small brass chain which hung in loops over the entire surface and the tuning fork of the oscillator was loaded with a piece of hard wax. This loading prevented any vibration of the diaphragm and the closing and opening of the control key resulted in two distinct though rather weak clicks. Fifty presentations of the clicks to each of the 4 animals, each pair being spaced to include 6 complete respiration cycles, failed to elicit any recordable change in the breathing rhythm.

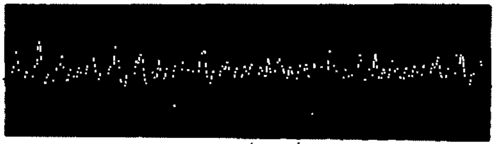

FIG. 1

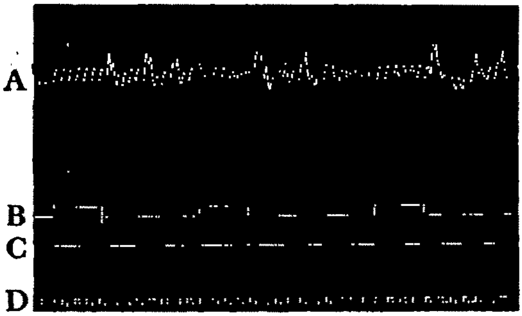

FIG. 2

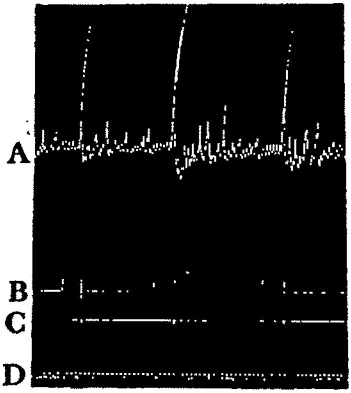

FIG. 3

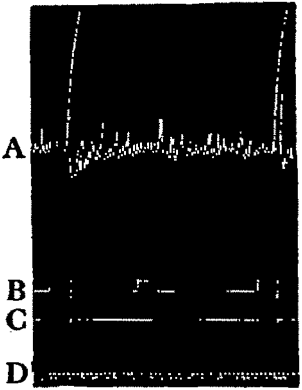

FIG. 4

A. Respiration record. B. Record of tone presentations. C. Record of electrical stimulations. D. Time interval, 1 sec.

FIG. 1. Sample record of undisturbed respiration.

FIG. 2. Note the regularity of the respiration represented by the part of the record coincidental with the record of the presentation of the tones. An inspiration of wide amplitude occurs at the cessation of the tone.

FIG. 3. The 'lullaby' effect is obvious in the part of the respiration record coincidental with the sounding of the tones.

FIG. 4. Presentation of the tone without the electrical stimulation is correlated with the 'lullaby' effect, which is followed by a respiration cycle of wide amplitude representing the true conditioned response.

After the conditioned response had been well established in the 4 animals, namely by 200 tone-shock presentations after its initial appearance, a test was made with each of them to determine whether the response was specific for a particular intensity of the conditioning tone. The 3 intensities available with the 600-cycle oscillator were presented in haphazard order with and without the shock. Ten tones of each intensity were presented to each animal. With the 3 intensities, when the shock accompanied the tone, the 'lullaby effect' always appeared in the breathing curve. When the shock was left out of the presentation the 'lullaby effect' accompanied the sounding of the tone and the conditioned response occurred at the termination of the tone.

TABLE I

NUMBER OF TONE-SHOCK PRESENTATIONS PRECEDING THE APPEARANCE OF THE INDICATORS OF CONDITIONING IN THE FOUR ANIMALS

Animal	'Lullaby effect'	End response
1	263	489
2	278	512
3	251	632
4	238	540

The 600-cycle oscillator was replaced by the 1000-cycle oscillator to determine the specificity of the conditioning as regards pitch. The intensity of the test tone (1000 d.v./s.) was approximately equal to that of the conditioning tone (600 d.v./s.). Each animal was presented with 25 tones, 10 of which were accompanied by the shock, and so spaced that the periods between presentations varied in a haphazard order from 10 sec. to 5 min. Except for the difference in frequency of the tone the conditions in this test were identical with those prevailing while the animals were being conditioned. No recordable change in the breathing of any of the animals was correlated with the presentations of the new test tone.

SUMMARY

The discrimination maze-method is useless for the solution of such problems as tonal discrimination, because it does not offer adequate possibility of control. Past experiments conducted by this method furnish no genuine evidence that animals are incapable of responding to pure tones. The present experiment demonstrates by the establishment of specific conditioned responses and objective records of significant movements, that the guinea pig possesses a mechanism which mediates the reception of relatively pure tones.

The data presented indicate that a generalized conditioned response to all available intensities of the conditioning frequency had been established. The specificity of the conditioning for a particular pitch is shown by the failure of the test tone of 1000 d.v./s to elicit responses from any of the animals.

A MECHANICAL EXPLANATION OF THE LAW OF EFFECT

By J. M. Stephens, Turtleford, Saskatchewan, Canada

As this paper attempts to present both the description of a learning machine and of a theory of learning, it might be helpful to insert a statement of the inter-relations between the apparatus and the theory.

As a rule, when analysis has reached a certain point, synthesis becomes possible. As yet synthetic protoplasm has not appeared. Many of the current explanations of learning, however, have analyzed the phenomenon independently of protoplasm. In other words, learning has so often been explained in non-protoplasmic terms, and the analysis has been carried so far that a non-protoplasmic synthesis would seem possible. In many cases learning has been analyzed into the elements of conduction bodies, connections and variable resistances. We have these elements outside of protoplasm. Consequently, if the analysis is valid we should be able to synthesize these elements so as to produce non-protoplasmic learning.

The above conception occurred to me as a means of evaluating those expositions of learning couched in neurological terms. This test of possible synthesis had, at first, only theoretical interest. To my surprise, however, I found that some of the analyses presented startling possibilities of mechanical synthesis. Of course modifications were necessary, so the theories had to be slightly altered or at least differently emphasized, before they could be applied to this particular attempt.

I have given the description of the machine first and then tried to point out the principles of its operation. Following this, I have stated the theory in more physiological terms—ignoring for the time the older theories of which it is an adaptation. I have then tried to indicate the theories from which the present conception was generated, (through the medium of the synthetic test) and to point out the modifications.

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Confusion may be prevented by reference to some points which might be considered departures from the traditional discussion. I have had to narrow the conception of the S–R bond, especially of the R. Here I have had to consider that a movement which reaches a certain point is distinct from one which reaches a different point. Thus, in the case of the classical baby reaching for the classical orange, R_1 and R_2 might both indicate reaching 'for' the orange, but would still be considered here as separate movements unless, in each case, the hand was placed in exactly the same position with reference to the body. It will be seen that this conception is necessary in the case of the machine.

In addition to the above limitation, the idea of random movements had to be dropped. I could not plan a machine capable of producing random movements, though it would be easy to construct a machine which appeared to move at random. Hence I was forced to provide a means whereby a machine could achieve the same results with 'determined' movements. To be consistent I have applied this explanation to animate learning.

This application has been adhered to throughout. I have tried to use no explanation of animate learning which could not be considered to work in a machine. I have considered protoplasmic organisms as very complicated machines and nothing further.

THE MACHINE

The machine is shown in Fig. 1. Suppose a finger is placed on the wire A, pressing this wire into contact with the bare wire of the cage. A current will pass from the cage to A, down to Y, along the wire indicated by - - - - - - - - - - through battery D to L, to J, to R, along the wire indicated by ' ' ' ' ' ' ' ' ' ' ' ' to M, through M and to the cage at F_2. This will make a complete circuit around Magnet M which will be energized. This would attract the cube B with its system of bars toward M and B_1 would hit the finger which presses on A. As the machine was designed to learn to hit the disturbing finger no learning is necessary in this case.

Suppose the finger is moved and is placed on C, pressing this wire into contact with the cage. Thus a current will pass from the cage to C along the wire- - - - - - - - - - -through battery D to L, to J, to R, along the wire ' ' ' ' ' ' ' ' ' ' ' to M, to the cage, thus completing the circuit. Again M will be energized and B with its bars will move toward M. Now, however, there is no finger at A to stop the movement of B, and the bar B_2 will hit the bare wire of the cage. This completes a new circuit. We can trace this new circuit from B_2 to the cage, from the leg F_2 of the cage, along the wire —— · · · —— · · · through battery E around magnet P to T and back to B_2, making a complete circuit around P and energizing it. When P becomes a magnet it will attract the needle switch

J towards its (J's) zenith. The momentum of J, however, will carry it slightly past the zenith.[1] Meanwhile when J left the terminal R the circuit around M would be broken, M would lose its energy, B would fall away from the cage breaking the circuit around P. P would lose its energy and J would fall on the terminal U, as the momentum has carried it to that side of its zenith.

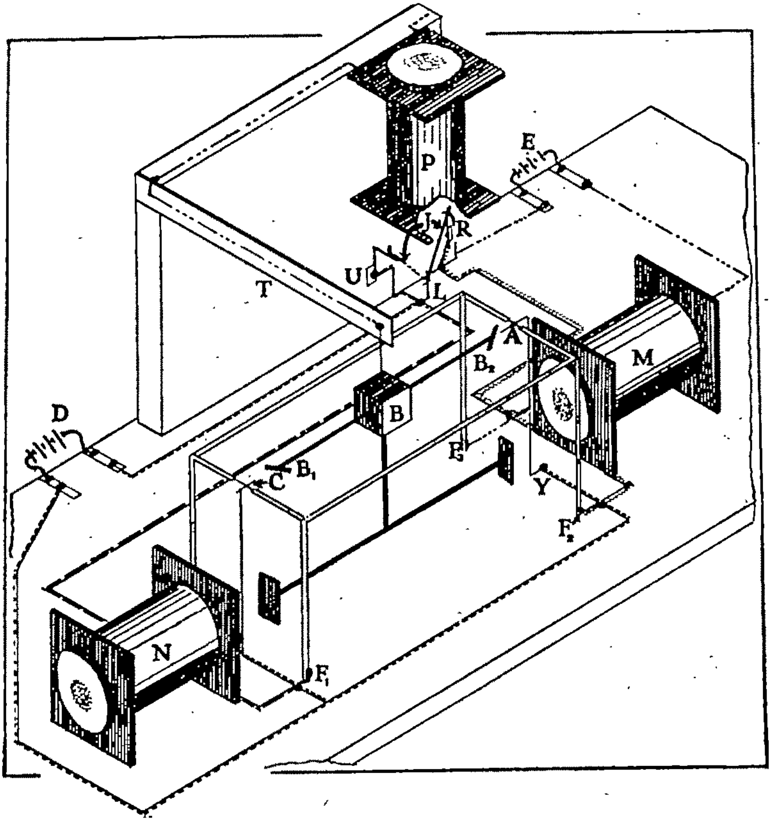

FIG. I

THE MACHINE

The Cage is shown between A and C. This consists of a quadrilateral of insulated wire. The insulation is removed at A and C. There is a clear electrical circuit from any part of the cage to any other part.

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[1] To insure that J will be pulled past its zenith before P loses its energy, I have found it necessary to complicate J by mounting a light compass needle and a light copper wire independently on L so that each is attached at one end by a universal joint to L and the other end swings freely between R and U. The wire and the needle are tied together by a silk thread of such a length that the needle can be just at the zenith of the swing while the wire is still resting on either R or U. In this way the wire will not be disturbed till the needle is carried away by its own momentum past the zenith. At this point the wire will be pulled away, ultimately breaking the circuit around P and allowing both needle and wire to fall over on the opposite side.

All this will take but a fraction of a second. The finger is still pressing at C, but the connection at J has been altered. The current will now pass from the cage to C through D to L, to J, to U, along wire ——— ——— ——— ——— to N and back to the cage, completing the circuit around N. N will attract B and the bar B_1 will hit the finger. The finger will prevent any contact between B_1 and the cage and hence there will be no circuit around P and no disturbance of J which will continue to rest on U. As long as J continues to rest on U any current along the wire - - - - - - - - -(from *either* A or C) will find an outlet through U and N.

Consequently, if the finger is placed at C again, B will hit it immediately and will continue to do so until contact is made at some other point. The machine will always make that 'response' which proved successful the last time it was 'stimulated.' Could we not say that the machine has learned to hit the finger? Here we have a perfectly mechanistic means whereby the successful 'response' may be 'selected.' The design of the machine is such that any movement which does not hit the finger (hits the cage) will be changed.

If we consider the machine as an organism which is subject to stimulation by the outside world through only one pathway—susceptible to only one stimulus—it seems as if we have a typical example of animate learning. The action of B might well be the action of a blindfolded man trying to touch the hand that is placed near him. It will be seen that it is only the principle of learning which has been here illustrated. To make a machine which would respond to any one of any given number of stimuli by any one of any number of responses, would involve no new principle and is quite possible for any one with the funds at his disposal.

The laws which govern the learning of the machine are relatively simple. We must think of a stimulus having *potential* connection with two responses. We must further think of the resistances of these two connections being variable. In the machine one connection is between L and R and the other between L and U. The resistances of these two connections vary according as J rests on R or U. If, when the machine has been stimulated, the resistance between L and R is less than that of the other connection, the response will take place through R and M. If that response produces one effect (hits the finger) the resistance of the connection will not be increased. If that response produces another effect (hits the wire) the resistance of that connection—the connection which caused the 'failing' response—will be increased.

We may say, then, that *one effect of a response will increase the resistance (relative) of the connection which caused it, whereas another effect will not increase the resistance of that connection.* This statement summarizes the activity of the machine, but as the machine was constructed largely as an illustration of that statement, it remains to be shown how that summary can be derived from animate learning and what claims that statement has in relation to any other accounts of learning.

We will assume that animate learning is of the trial and error type. Let us imagine a case of a blindfolded subject attempting to strike a bell which lies on the table. We will distort the case by supposing that the stimulus is constant or identical in each case, and that the subject is only susceptible to this stimulus, and to the sound of the bell and the sound of the impact on the table. The probabilities will be something like the following.

Stimulus	*Response*	*Effect*
S_1	R_1 (hits table)	Sound of impact on table
S_1	R_2 (hits table but in different place)	Sound of impact on table
S_1	R_3 (hits bell)	Sound of bell
S_1	R_3 (hits bell)	Sound of bell
Bell is moved unknown to subject.		
S_1	R_3 (same movement but hits table)	Sound of impact on table
S_1	R_4 (hits bell)	Sound of bell
S_1	R_4 (hits bell)	Sound of bell

A summary of the above will show that any response which is followed by the sound of the table is altered on the next presentation of the stimulus, while any response which is followed by the sound of the bell is repeated on the next presentation of the stimulus. This is very similar to the statement which was made with regard to the machine.

If, in the above example of learning, we can assume that the stimulus is constant, we are almost driven to say that the effect of the response changed the connection which caused the response. Moreover, I believe we must assume that the *determining element* of the stimulus was constant, if we have any learning as it is ordinarily understood.

The necessity for this assumption will be clearer from another case of learning. The stimulus can never be the entire situation, yet, in many organisms, it produces a response of such constancy that we must assume it to be a constant determiner of that response in those cases. Yet at one time that constant stimulus produced varying responses until one was selected because it produced the required effect.

Now if there is a constant determining element in the stimulus and a varying response, we must assume a varying connection. Moreover, if a response which appeared to that stimulus does not again appear to the same stimulus, there must have been an increase relatively in the resistance of the connection which caused that response.

Consequently, there seems to be evidence that the conception of learning synthesized into the machine is not only derived from animate learning, but seems necessary in the explanation of animate learning. The exact mechanisms by which such changes in the resistance of connections is brought about is far from determined. In Fig. 2 a possible means—after the manner of the arrangement in the machine—is suggested. Here the sensory neurone S_1 is represented as in potential connection with the motor neurones R_1, R_2, and R_3 by means of connections C_1, C_2, and C_3 respectively. We need only imagine that the stimulation of any K would increase the resistance of the adjacent C. Thus the impulse would change its outlet until it happened to effect a response which did not stimulate K. Of course, the absence of the normal effect could be considered to have the same result as the stimulation of pain neurones. Any suggestion of the actual nervous mechanism which secures this would be very hypothetical indeed and probably better not attempted. The mechanism, however, is quite conceivable.

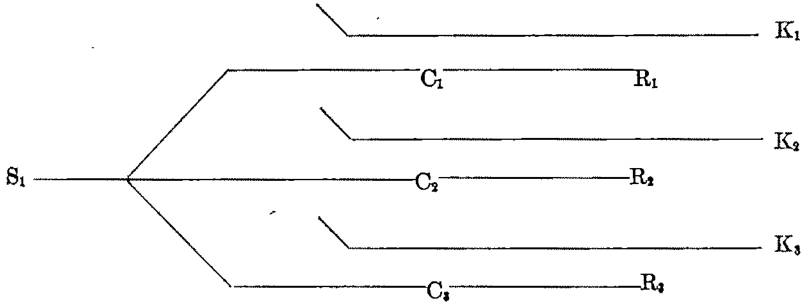

FIG. 2
AN HYPOTHETICAL ARRANGEMENT OF NEURONES

So far the accepted expositions of learning have been ignored. I believe most of these would explain the hypothetical case of the subject learning to hit the bell as follows. R_1 and R_2 are mere 'random' movements which are unsuccessful and are not stamped in. By pure chance R_3 is successful and is stamped in. Now the resistance of the connection between S_1 and R_3 will be decreased by use or frequency. Other explanations suggest that R_3 will always be the most recent and the most frequent and these factors alone would explain the learning.

The explanation given here differs from the above in several matters. In the accepted positions the fact that the responses vary has received very little emphasis. The varying responses have been taken for granted and the whole attention has been focused on the process of the selection of one of these. But is not this only one half the problem? There is some ground for the assertion that this variety of responses to a *constant* stimulus constitutes a very real phenomenon and an essential part of the total phenomenon of learning. Moreover, it is not unlikely that there should be an intimate relationship between the agent which causes the responses to vary in the first place and the agent which causes the varying to cease—or, in other words, which causes one response to be selected.

In view of the above I have tried to regard the variety of response and the constancy of response as converse aspects of the same phenomenon. A review of the problem of the subject striking the bell will show a series of varying responses followed by a constant response. If we can find an agent which will account for the variety, then constancy of the response could be expected whenever that agent ceased to operate.

If S_1 first produces R_1 and then R_2, we must assume that there has been a change in the relative resistances of the connections—that the resistance of the connection between S_1 and R_1 has become relatively greater. We find this also happens in the case of S_1-R_2. As stated in the summary, the resistance (relative) of any connection will *increase* with the operation of the connection, *provided it fails to achieve a certain effect.* Either the lack of that 'certain' effect produces that increase in the resistance, or the presence of the 'certain' effect prevents the operation itself from increasing its own resistance.

This seems to give a more complete account of the whole process. Moreover, I believe that it is as easy to think of learning taking place through the increase of the resistance of inhibiting connections as through the decrease of the resistance of the particular connection. This will be clearer by again referring to Fig. 2.

If the nerve activity tends to go via C_1 at first and does not do so the next time, we must suppose one of two things has happened. Either the resistance of C_2 and C_3 have been so decreased that the energy goes through those connections rather than through C_1 *or* the resistance of C_1 has been increased. This latter conception is the simpler. Moreover, it is the only conception which will explain the facts *unless the neural energy depends on a difference in potential.* That is to say, if the nerve impulse is electrical or hydraulic in nature, a great decrease in the resistance of any one connection would drain *most* of the energy from the other connections. It would never drain all. If, however, as seems more probable, the nerve energy is not electrical or hydraulic, then no decrease in the resistance of C_2 or C_3 would ever stop the transmission of an impulse across C_1. If the neurones in Fig. 2 represented trains of gunpowder, then the only way we could prevent the transmission of the impulse to R_1 would be by actually interfering with the resistance of C_1.

The substance of the above may be given thus: The phenomenon of varying responses is as essential to learning as is the phenomenon of the selection of one of those responses. The phenomenon of variation can never be explained by frequency, recency, etc., but only by the conception of an increase[2] in the relative resistance of all connections not achieving a certain effect. This conception besides accounting for the variety of the responses, will also account for the constancy of any response that *does* achieve the certain effect. The action of the machine shows that it is quite possible to provide a mechanism which will increase the resistance of every response which fails to achieve a certain effect—to provide a mechanism which will produce learning.

The present account differs from the traditional explanations in its lack of emphasis on frequency and recency. Once there is a causal agency to stamp a response in or to stamp it out, it follows that two applications of that agency will have more effect than one application. This I understand to be the principle of frequency.

The nature of the discussion prevents reference to the many conceptions of learning that have been utilized. As the study was instigated, however, by an examination of three theories, these should receive special mention.

The similarity of this conception to that of Thorndike is apparent.[3] In general this conception may be thought of as a mechanistic interpretation of

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[2] While this paper was in process of revision an article by Professor Knight Dunlap (A revision of the fundamental laws of habit formation, *Science*, 67, 1928, 360) appeared, in which he puts forward the view that the operation of a synapse may, and in some cases probably does, *increase* the resistance of the synapse.

[3] E. L. Thorndike, *Educational Psychology: The Original Nature of Man*, 1913, I, 124-7; The law of effect, this JOURNAL, 39, 1928, 212-222.

Thorndike's position. The exceptions which should be noted are those mentioned earlier in connection with the traditional explanation, and the omission of such terms as 'satisfaction' and 'annoyance' in the present account. Thorndike himself defines satisfaction and annoyance in strictly neurological (mechanistic?) terms. It was this neurological definition which constituted the chief basis for the present mechanical synthesis. Thorndike, however, does not seem to regard the neural condition in itself as the determiner. Except for those limitations imposed by a strictly mechanistic interpretation, this account follows Thorndike throughout—to the best of my comprehension of his position.

The same may be said of Carr's position.[4] I know of nothing herein contained which differs greatly from Carr's explanation with the general exceptions given above, and with the exception of his conception of the *intensity* of the sensory consequences. One of the main points of the present conception is the influence of the sensory consequences in producing a variety of responses and in selecting the particular response. Where Carr attributes the selective agency to the *intensity* of the consequences, I have been only able to suggest that one consequence stamps out and the other does not. I believe that neither Carr's nor Thorndike's position has been greatly distorted by this attempt to synthesize them into a mechanical application.

The two positions mentioned above have been considered from the viewpoint of Watson's criticism.[5] The whole outlook of the problem was largely determined by his rigid applications of the law of parsimony. As indicated above, this position does not emphasize the factors of recency and frequency to the extent which Watson has, but considers them as secondary to the operation of a resistance producing agency.

There have been many other explanations which have entered here. A discussion of them—especially those of Peterson[6] and Haggerty[7]—within these limits would, however, be most unjust, although I would like to consider briefly the conditioned response. By this means Symonds explains a great deal of learning.[8] He then states that the law of effect plays no part in the conditioned response. In other words the law of effect plays no part in the explanation of a great deal of learning. This would be unfortunate for our theory which tries to show that all learning is explained by the law of effect. It would be even more disastrous for the conditioned response which, I believe, is denied all semblance of an explanation if the law of effect is rigorously excluded.

In one case of conditioning a bell is rung near a dog. Shortly afterward meat is presented. After a sufficient number of repetitions the sound of the bell alone produces salivation.

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[4] H. A. Carr, Principles of selection in animal learning, *Psychol. Rev.*, 21, 1914, 157-165; The distribution and elimination of errors in the maze, *J. Animal Behav.*, 7, 1917, 145-160.

[5] J. B. Watson, *Behavior: An Introduction to Comparative Psychology*, 1914, 251 ff.

[6] Joseph Peterson, Completeness of response as an explanation principle in learning, *Psychol. Rev.*, 23, 1916, 153-162; Forced adjustment *vs.* association in constructive learning and thinking, this JOURNAL, 39, 1927, 264-282.

[7] M. E. Haggerty, Laws of learning, *Psychol. Rev.*, 20, 1913, 411-422.

[8] P. M. Symonds, Laws of learning, *J. Educ. Psychol.*, 18, 1927, 405-413.

In this explanation we must assume that every response the dog will ever make to that stimulus is already in potential connection with that stimulus, or in a position so that it can be connected. Then S produces R_1—an attempt to run away, for example. The appropriate effects are not forthcoming and that connection is stamped out. Now S produces R_2 which we will suppose is also stamped out. Now suppose S produces R_3—preparation for food taking. The appropriate effects (such as the stimulation of taste neurones) ensue and this response is *not* stamped out. Whenever the appropriate results fail to follow this response it is very rapidly stamped out.[9]

In case it should be objected that the animal must make an enormous number of reactions in a very short time, we might point out that most of the experimentally produced conditioned responses so far reported have had to do with either food-taking or fear (avoidance) reactions. It is not unreasonable to suppose that the animal should be so organized that his first response to a rather novel situation should be of an avoiding nature, and if that failed to produce the appropriate effect, the next response would be concerned with food taking. Certainly an animal with some such nervous organization would seem to have an advantage over those not so organized.

SUMMARY

The real phenomenon of learning is considered to lie in the fact that one stimulus will produce several different responses of which one will be selected to follow the stimulus almost invariably. It is held that this altering and selective force is associated with the effect of the response.

A mechanistic law of learning reads as follows: A connection which results in one effect (probably biologically useful) will not be stamped out and may be stamped in. A connection failing to produce this type of effect, or producing another type (probably biologically harmful) will tend to be stamped out.

This conception of learning was derived from the analysis of a learning apparatus and has been synthesized to produce a learning apparatus. While this does not prove the theory, it is considered as an argument in its favor.

The nature of the discussion has not been conducive to a just emphasis on the rather lengthy history of the problem, but the chief purpose of this paper has been realized. Its object was to show that the current conceptions of learning—notably those of Carr and Thorndike—are capable of such thorough-going mechanistic interpretation that a mechanical application of these mechanized principles is a possible and an accomplished fact.

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[9] I. P. Pavlov, *Conditioned Reflexes*, 1927, 49-62.

LEARNING THE RELATION OF OPPOSITION

By George Kreezer and Karl M. Dallenbach, Cornell University

A few years ago the senior author of this study reported in a note[1] the results of a little experiment on the learning of the meaning of opposition by two children, 3 and 6 years old respectively. Neither *S* understood the meaning of opposition at the beginning of the experiment, but the older learned it immediately upon gaining 'insight' into the nature of the problem. The younger *S* was, however, unable to learn the meaning in spite of numerous repetitions of illustrative examples.

The object of the present study is to extend the investigation to 100 *S*s, and, by a statistical treatment of the results, to answer the following questions: (1) at what age do children in general know the meaning of opposition; (2) how do the children who do not know the meaning learn it—do they learn it gradually, in accordance with the 'trial and error' theories of learning, or suddenly with the appearance of 'insight;'[2] and (3) at what age are children in general capable of learning the meaning.

Subjects. The *S*s, 100 in number (43 girls and 57 boys), were selected from the kindergartens and the first and second grades of the Belle Sherman, St. John's and East Hill schools of Ithaca, N. Y.[3] Chronological age was the only basis of selection. We chose 20 *S*s from every one of the following 5 age-groups: 5·0-5·6, 5·7-5·12, 6·0-6·6, 6·7-6·12, 7·0-7·6.[4]

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[*] Accepted for publication February 14, 1928.

[1] K. M. Dallenbach, A note on the immediacy of understanding a relation, *Psychol. Forsch.*, 7, 1926, 268-269.

[2] Köhler's argument in his *Mentality of Apes* rests upon evidence from but few cases. In reply to the criticism that he should have applied the statistical method to his results, he says: "To my mind this means an inability to see the problems which I tried to treat in a preliminary way" (*Gestalt Psychology*, 1929, 52 f.). We agree with Köhler; qualitative work must precede the quantitative; but after the 'preliminary' work has been done quantitative work should follow.

[3] We wish to express our appreciation for the coöperation of the teachers, Miss Ella E. Richards, Miss Freda R. Slater, and Mrs. Katherine O. Williams, and of Dr. F. D. Boynton, Superintendent of the Ithaca Public Schools.

[4] The intellectual and social status of the parents of our *S*s is probably weighted toward the upper extremes. Thirty-six of our *S*s' fathers are engaged in professional occupations (teachers, doctors, lawyers), 34 in business, 25 in skilled labor, and 5 in unskilled labor.

Method and procedure. The *S*s were taken one at a time to the room of the experimenter. The child was asked his name and a few other questions to draw his interest and to gain his confidence, and then he was asked whether he liked to play games. After the invariable affirmative answer, *E*'s procedure was verbatim as follows:

(A) "We are now going to play the game of opposites. Do you know what opposite means?"

(B) If the answer was no, *E* said, "Yes you do, tell me what is the opposite of *good*."

If the answer was yes, *E* said, "All right, tell me what is the opposite of *good*."

(C) If *S*'s reply was incorrect, *E* said, "No, you are wrong, that is not the opposite of good. Now tell me what is the opposite of *big*."

If *S*'s reply was correct, *E* said, "That's right, now tell me what is the opposite of *big*."

(D) If *S*'s reply was incorrect, *E* said, "No, you are wrong. Now listen, the opposite of *good* is *bad*, the opposite of *big* is *little*. Do you understand?"

If *S*'s reply was correct, *E* said, "That's right, the opposite of *good* is *bad*, the opposite of *big* is *little*. Do you understand?"

(E) If the answer to the question in (D) was no, *E* said, "Well, I'll see if you do. Tell me what is the opposite of *white* (*short, fat, wet, glad, hard, in, slow, weak, open, heavy, up, smooth, little, sleep, shut, strong, quick, out, soft, sad, dry, thin, long, black*)."

If the answer was yes, *E* said, "All right, I'll see if you do. Tell me what is the opposite of [the 25 words listed above]."

(F) As the words were given to *S* and he returned his answers *E* simply replied 'right' or 'wrong.' If *S* did not show by his answers to the first 10 words of the test list that he understood the relationship, *E* again told him the opposite of *good* and of *big*, and then asked him a second time for the opposites of the first 10 words, saying 'right' when a correct answer was returned and supplying the correct answer when an error was made. After this additional instruction in the meaning of opposition, *E* continued, without correcting or commenting upon *S*'s replies, to ask for the opposites of the 15 remaining words in the list.

An *S* was credited with having learned the meaning of opposition if he continued, after a correct response to some word among the first ten, to give correct reponses to the remaining words in the test series. All the *S*s who demonstrated that they had learned the relationship did not, to be sure, give antonyms in every instance to the test words, but as we were not concerned with a vocabulary test it was not necessary for our purposes that antonyms be given. All that we required was evidence that the *S*s, as demonstrated by their replies to the 25 test words, understood the nature of opposition. The answers 'not smooth,' or 'un-smooth,' to the word *smooth*, for example, were as significant for our purposes as the antonym 'rough.'

This procedure, identical with the one used in the preliminary study mentioned above, was calculated to instill confidence into the *S*s and to prompt them to respond. Nothing was done or said that discouraged the child. *E*'s 'wrong,' which was necessary for the *S*'s instruction, was not said disparagingly or reproachfully. We wished the failures of the children to learn the meaning of opposition to be real failures and not chargeable to discouragement, bashfulness, or fear.

Results. (*1*) *Age at which meaning is understood.* We give in Table I the results of the 6 different parts of the procedure for every one of the 5 age-groups. In the first row, opposite stub A, we give the answers that we received in part (A) in reply to the question "Do you know what opposite means?" One *S* in the 5·7-5·12 year age-group, 5 in the 6·7-6·12 year age-group, and 4 in the year 7·0-7·6 age-group replied in the affirmative. But, as the responses obtained from parts (B) and (C) of the experiment show, the *S* in the 5·7-5·12 year age-group and one of those in the 6·7-6·12 year age-group mistakenly reported that they understood, for they failed in their performance to give the opposites of *good* and *big*. The data in these parts show furthermore that 7 *S*s in the 7·0-7·6 year age-group—3 more than those who reported that they understood the meaning—demonstrated by their performance that they understood the relation. It is ap-

TABLE I

Showing the Results of the Various Parts of the Experiment for the Different Age-Groups

		Age-group					
Part	Answer	5·0-5·6	5·7-5·12	6·0-6·6	6·7-6·12	7·0-7·6	Total
A	yes	0	1	0	5	4	10
	no	20	19	20	15	16	90
B	right	0	0	0	4	7	11
	wrong	20	20	20	16	13	89
C	right	2	0	0	4	7	13
	wrong	18	20	20	16	13	87
D	yes	13	15	14	15	20	77
	no	7	5	6	5	0	23
E	correct	7	10	8	10	16	51
F	correct	1	2	7	6	2	18
Total E and F		8	12	15	16	18	69
Percent successful		40	60	75	80	90	
Total number unsuccessful		12	8	5	4	2	31

parent, therefore, that 3 *S*s understood the relation but not the formal name for it. On the basis of these data it appears that children under 6.6 years of age do not understand the meaning of opposition, and that approximately a fifth of the 6·7-6·12 year age-group and a third of the 7·0-7·6 year age-group do understand it. Our age-groups were not sufficiently large nor did they sufficiently extend into the upper years of childhood to permit us to state definitely at what ages children in general understand the meaning of opposition, but it is apparent from the trend of our results that they probably learn it without specific instruction by the end of their eighth year, or the beginning of the ninth.

(*2*) *Manner in which the meaning is learned.* The data of part (D) of the experiment, given in the fourth row of Table I, show the number of *S*s in the different age-groups who reported that they had learned the meaning of opposition after it was illustrated in parts (B) and (C). As shown by their actual performances in (E), however, more *S*s thought they understood it than were actually able to demonstrate it. The number in every age-group in (E) is smaller than the number of 'yes'-reports at the corresponding age-groups in (D).

The differences between the numbers for every age-group in (E) and the numbers opposite 'right' for the corresponding age-groups in (B) represent the number of *S*s who suddenly, after being given but two illustrative examples, learned the meaning of opposition. Only 11 *S*s, 4 and 7 respectively in the two oldest age-groups, demonstrated at the beginning of the experiment—in (B)—that they understood the relation; 51 *S*s—40 additional—demonstrated in (E) that they understood it.

Our data give evidence that two *S*s in the 5·0-5·6 year age-group learned the relation after one correction. They reported in part (A) that they did not know the meaning of opposite; they failed to give the opposite of *good* in part (B); but in part (C) they responded to *big* with *little.* The replies of these 2 *S*s might of course have been mere chance associations—for it is a bit surprising that the only instances of learning the relation from one apprisal of error should have appeared among the *S*s in our youngest age-group—but these *S*s replied in part (D) that they understood the relation, and they demonstrated that they did in part (E). If their success in part (C) was due to chance association they must have learned it immediately afterward, for their success in part (E) could not have been due to chance. It appears, therefore, as though they actually did learn the relation from one example.

The data in Table I, in the row opposite stub F, show the number of *Ss*, and their distribution among the different age-groups, who learned the relation in part (F) of the experiment. Eighteen *Ss*—1, 2, 7, 6, and 2 respectively in the 5 age-groups ranged from the youngest to the oldest—demonstrated, after the illustrative examples were repeated a second time and their replies corrected for the first few words of the test list, that they understood the relation by giving the opposites of the remaining words of the list. None of these *Ss* required correction after the fifth word.

All the *Ss* who learned the relation learned it immediately with a sudden acquisition of insight. Even the *Ss* who learned the relation in part (F) of the experiment, where an opportunity was offered for gradual acquisition, exhibited the same sudden shift from free chance associations to associations determined by the relation of opposition. None of our *Ss* exhibited a gradual acquisition or understanding of the relation. The curve illustrating their progress in learning dropped abruptly from 100% failures to 0%.

Thirty-one *Ss*, distributed among the different age-groups as shown in Table I opposite the stub 'Total unsuccessful,' were unable to learn the relation of opposition with the amount of instruction that we gave them. Their data are significant, however, in showing how the relation of opposition is learned. None of these *Ss* showed any progress toward learning; they were as far from success at the end of the experiment as they were at the beginning. They did not acquire the insight requisite to learning, consequently they did not learn. The curve illustrating their progress remained as high at the end as at the beginning.

None of the *Ss* who learned the meaning of opposition showed during the experiment any tendency of retrogression or forgetting. The relation once learned persisted from the moment that insight was gained; there were no momentary lapses or slips back to the unlearned state. Learning by insight appears to be immediate—a case of 'all or none'—and also permanent.

How may we explain these results? Certainly not by the 'trial-and-error' theories of learning. Neither Thorndike's so-called law of exercise,[5] bulwarked though it be by the subsidiary law of effect,[6] nor Watson's alleged law of frequency,[7] supported though it be by the important law of recency, will explain our results. There was no opportunity in our experiments for the operation of these laws, and yet 58 of our *S*s learned the relation.

According to Thorndike, the ability to make a correct response to a particular stimulus, R_1 to S_1, can *in general* be acquired only if the stimulus-response connection $S_1 \rightarrow R_1$ has occurred and has been 'stamped in' during the learning period. Our results cannot, however, be explained by this hypothetical structure, for during the interim between our *S*s' failures and successes not only had the correct responses not been 'stamped in' but they had not occurred. Consider, for instance, the *S*s who demonstrated in part (E) of the experiment that they had learned the relation of opposition. The specific responses to the 25 test words were not given them, and yet after two illustrative examples they correctly responded to all the words in the test list. Or, if it be argued that these *S*s, in spite of their denials and their failures with *good* and *big*, al- y understood the relation of opposition, and that their results consequence cannot be regarded as pertinent to the problem o ning, take the results of the 18 *S*s who failed in part (E) of th periment and succeeded in part (F). There can be no que n regarding the learning of the relation by these *S*s. Ever ne of them succeeded, without having the correct respons urnished them, in giving correct responses to stimulus-words that they had previously missed. The law of exercise cannot explain these results.

The qualification "in general," italicized in the statement of the general law given above, admits of exceptions which Thorndike attempts to cover by his auxiliary law of assimilation or analogy. Thorndike states "that to any situations which have no special original or acquired response of their own, the response made will be that which by original or acquired nature is connected with some situation which they resemble."[8] Even if we regard the situations in all of our test words as analogous—for example, the situation aroused when the opposite of *white* is asked for as analogous to the situation aroused when the opposite of *sad* is asked for—the law of analogy cannot account for the exceptions to the general law met in our experiment, for the *S* should, according to this law of analogy, give as the response to every new word the response which he had given to some word earlier in the list. The law of analogy could lead in our experiment only to wrong responses; it obviously is unable to account for our results.

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[5] E. L. Thorndike, *Educational Psychology*, 1913, i, 171 ff; ii, 2 f.

[6] E. L. Thorndike, The law of effect, this JOURNAL, 39, 1927, 212-222.

[7] J. B. Watson, *Psychology from the Standpoint of a Behaviorist*, 1919, 290-367.

[8] Thorndike, *op. cit.*, ii, 15.

Watson's 'laws' of habit fixation are also inadequate to explain our results. His law of frequency, which is similar to Thorndike's law of exercise, fails for the same reason that the law of exercise fails: up to the moment that correct responses were given they had never occurred; frequency of response should have led to a continuation of the incorrect responses.

Watson's supporting law of recency does not help; it merely compounds the failure of his treatment of learning. Up to the moment that our *S*s gave correct responses all of their responses had been incorrect; after that moment all of their responses were correct. Recency alone should have led to a continuation of the incorrect responses.[9]

In terms of Thorndike's and Watson's theories of learning, the correct responses of the *S*s who learned the relation of opposition in our experiment can be attributed only to chance. Chance might account for sporadic occurrences of correct responses, but one can hardly resort to it to account for the *continued* occurrence of correct responses. The sudden and continued success of our *S*s makes it clear that they had *learned* to respond correctly to the stimulus-words. The fatuousness of the 'chance explanation,' which is the only explanation of our results that can be given by Thorndike's and Watson's theories illustrates how totally inadequate to the problem their theories of learning are.

The results of this study show that our *S*s learned the relation of opposition suddenly, if they learned it at all, with the appearance of insight into the nature of the problem.[10] They learned the relation, and not a specific response to a specific stimulus; yet the learning of the relation enabled them to give correct responses to specific stimulus-words to which they had earlier in the experiment responded incorrectly.

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[9] Watson attributes much importance in the early acquisitions of the child to the conditioned reflex (*op. cit.*, 290, 341). By means of this mechanism a stimulus which is at first inadequate to evoke a certain response may become adequate if it occurs together with a stimulus that does evoke the response. It will suffice, in order to show that this mechanism has no bearing upon our problem, to point out that both the stimuli and the responses in our experiment were specific. There was no opportunity for conditioning to take place, and had it taken place it would invariably and of necessity have led to wrong responses.

(3) *Age at which children are capable of learning the relation of opposition.* Table I shows the number and percentage of *Ss* in every age-group who demonstrated during the experiments that they understood the meaning of opposition. The percentage of successes, the data show, varies directly with age and increases most rapidly near the 50% region. Since these frequencies follow a curve of distribution of the ogive form, it seemed feasible to employ the method of constant stimuli in calculating the probable age at which children will be capable of understanding the relation of opposition, particularly as the method has already been successfully applied to problems outside the sensory field for which it was especially developed. Williams used the method in calculating an associative limen,[11] Fernberger in computing the constant in the range of apprehension,[12] and Guilford and Dallenbach in determining the memory span.[13]

The value obtained by treating our data by the method of constant stimuli will correspond to the stimulus limen; it will indicate the age at which there is a probability of 0.5 that a child will earn the relation, *i.e.* it is the calculated age at which 50% of the children of a like social status will learn the relation and 50% will fail. The value we obtained from the computation was $L = 5.49$, $h = 0.52$. Since we represented the half-year age-groups in our computation by the medians, the calculated 'age-limen'—5.49—should likewise be considered as a median and the age-group to which it corresponds should be considered as extending from 5.24-5.74 years of age, *i.e.* from 5·3-5·9. Upon the basis of these results we feel justified in concluding that children of the 5·3-5·9 year age-group, of a social status similar to that of our group, have, with the amount of instruction that we gave our *S*s, a probability of 0.5 of learning the relation of opposition.

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[10] For further references to this type of learning see the discussion by K. Koffka, *The Growth of the Mind*, trans. by R. M. Ogden, 1924, 175-181, 214-219. Cf. also H. Helson, Insight in the white rat, *J. Exper. Psychol.*, 10, 1927, 378-396.

[11] H. D. Williams, On the calculation of an associative limen, this JOURNAL, 29, 1918, 219-226.

[12] S. W. Fernberger, A preliminary study of the range of visual apprehension, *ibid.*, 32, 1921, 121-133.

[13] J. P. Guilford and K. M. Dallenbach, The determination of memory span by the method of constant stimuli, *ibid.*, 36, 1925, 621-628.

Our problem in this section is similar to the one involved in the placement of mental tests in a test-scale. The question, 'what percentage of children should pass a test at the age for which it is to be considered standard,' which has long concerned those interested in the construction of mental tests, has been variously answered. Binet and Simon, for example, though they did not specifically discuss the question, showed from various incidental statements that they used a shifting standard which varied from 60% to 90% for the different age-groups. Goddard considered that a test was "misplaced in the scale if it is not answered by about 75% or more of those trying it."[14] Bobertag agreed, after a comprehensive study of the question, with Goddard's figure: "ein Test ist derjenigen Altersstufe zuzuweisen, auf der er bei etwa 75% aller normalen Kinder gelingt."[15] Terman and Childs "considered that two-thirds of the children of any age ought to pass a test to make the test characteristic of that age."[16] Kuhlmann at first accepted the 75% standard,[17] but later adopted a sliding standard which was centered about 60%,[18] and still later a standard which "ranges from nearly 100% at the lower end of the scale to about 50% at the upper end."[19] Terman, in his revision, likewise adopted a sliding standard ranging from about 77% at the lower age-groups to about 55% at the upper age-groups.[20] Otis, after a purely mathematical consideration of the question, concluded that "the age-gradation of any test can be none other than the age at which just 50% pass it."[21]

Without entering into an extended discussion of this question, which would carry us beyond the limits of this paper, let it suffice for us to say that we are in substantial agreement with Otis's mathematical argument. We believe that the age at which 50% of those examined pass a test is mathematically the only defensible criterion for the placement of the test. We furthermore believe that this age can best be calculated in accordance with the phi-gamma hypothesis and by the method of constant stimuli.

The advantages of employing the method of constant stimuli in the determination of the age at which 50% will pass a test are many. It gives an 'age-limen' that is unequivocally defined, and the definition rests upon the accepted psychophysical principle of the limen. All of the data, the successes and failures at every age, are taken into account. The method, furthermore, provides in h, the degree of precision, a measure of the amount of scatter. We repeat with Fernberger, "it is curious that investigators, working outside the field of sensation, have not oftener used the tools of psychophysics."[12]

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[14] H. H. Goddard, Two thousand normal children measured by the Binet measuring scale, *Ped. Sem.*, 18, 1911, 240.

[15] Otto Bobertag, Über Intelligenzprüfungen, *Zsch. f. angew. Psychol.*, 6, 1912, 505.

[16] L. M. Terman and H. G. Childs, A tentative revision and extension of the Binet-Simon measuring scale of intelligence, *J. Educ. Psychol.*, 3, 1912, 281.

[17] Fred Kuhlmann, A revision of the Binet-Simon system for measuring the intelligence of children, *J. Psycho-Asthenics, Monog. Suppl.*, 1, 1912, 14.

[18] Fred Kuhlmann, The results of one thousand public school children examined with a revision of the Binet-Simon Tests of intelligence, *J. Psycho-Asthenics*, 18, 1914, 248.

[19] Fred Kuhlmann, *A Handbook of Mental Tests*, 1922, 44.

[20] L. M. Terman, *The Stanford Revision and Extension of the Binet-Simon Scale for Measuring Intelligence*, 1917, 158.

[21] A. S. Otis, Some logical aspects of the Binet scale, *Psychol. Rev.*, 23, 1916, 178.

SUMMARY

The questions that we set ourselves may, within the limits of this study, be answered as follows.

(1) Children of the social status of our group learn the meaning of opposition without special instruction regarding it by the end of their eighth year or the beginning of the ninth.

(2) Children who do not know the meaning of opposition learn it with instruction, such as we gave our group, suddenly with the acquisition of 'insight.' None of our group exhibited a gradual acquisition or understanding of the relation. (a) Children who have learned the meaning of opposition do not forget it. Learning with insight is more stable and permanent than trial-and-error learning. (b) The trial-and-error theories of learning are unable to account for 'insightful' learning. The existence of this type of learning shows how totally inadequate to the problem of learning the trial-and-error theories are.

(3) Children of the 5·3-5·9 year age-group, of a social status similar to that of our group, have, with the amount of instruction that we gave our group, a probability of 0.5 of learning the relation of opposition.

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[12] S. W. Fernberger, *op. cit.*, 123.

JAN SWAMMERDAM: A STUDY IN THE HISTORY OF COMPARATIVE AND PHYSIOLOGICAL PSYCHOLOGY OF THE 17TH CENTURY

By Franklin Fearing, Northwestern University

The major contribution of the 17th century to the development of science was the recognition of observation and experiment as the ultimate test of scientific truth. In physical science, it was the century of Newton, Boyle, Kepler, and Leibnitz; in biological science it was the century of Malpighi, Borelli, Glisson, Willis, Van Helmont, Stenson, and, above all, Descartes and Harvey. Vesalius, in the previous century, had invaded the sacred mysteries of the human body itself, and now an intense curiosity prevailed in the field of the biological sciences regarding the structure and functioning of the bodily mechanisms. The attempt, by means of the new method of observation and experiment, to test the findings of mechanics and physics as applied to the functioning of living bodies, marks the beginning of modern biological science.

A striking manifestation of this curiosity about the functioning of bodily mechanisms is found in the large number of investigations of the mechanisms which initiate and control animal motion. The nature and uses of the disturbance transmitted along the nerve, the processes of muscular contraction, and the study of animal responses to specific stimuli were all subjects of observation and experiment. The extraordinary productivity of this field in a space of thirty years (1650–1680) is shown by the work of Glisson[1] on muscle irritability, Borelli,[2] Croone,[3] and Stenson[4]

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[1] Francis Glisson (1597–1677). See M. Foster, *The History of Physiology*, 1901, 287 ff.

[2] Alphonso Borelli (1608–1679). *De Motu Animalium*, 1680.

[3] William Croone, *De ratione Motus Musculorum*, 1664. Croone died in 1684 leaving a scheme for a series of lectures on muscular motion. This scheme was made financially possible by provisions in the will of Croone's widow. The first Croonian Lecture was not delivered until 1738.

[4] N. Stenson (1638–1686). *Elementorum Myologiae Specimen, seu Musculi descriptio geometrica*, 1667.

on the mechanics of muscular contraction, Mayow[5] on the nature of 'animal spirits,' and the observations of Boyle,[6] Du Verney,[7] and Descartes[8] on the involuntary responses of animals. Descartes' treatise *On Man*[9] went beyond the experimentally established knowledge of the time in an attempt to demonstrate that the body of man may be regarded as a machine operating according to physical laws. In a word, it was towards the study of the mechanisms of neuro-muscular adjustment of that machine that much of the biological research of the time was directed. Herein lies the peculiar historical significance of the 17th century to modern experimental psychology of psycho-motor phenomena.

Jan Swammerdam, a Dutch naturalist, using the comparative as well as the experimental method, made fundamental contributions to neuro-muscular physiology and physiological psychology. His studies in the field of comparative anatomy[10] of insects have tended to obscure his contributions to the physiology of muscle and nerve. In these entomological studies—which were not anatomical merely but included observations on behavior as well—the comparative point of view was stressed, as indicated, for example, by such chapter headings as "Man himself compared with insects, and with the frog," and "A particular treatise on the frog and its young, exhibiting its history, and comparing it with insects."[11] His insight into the problems of neural physiology, his use of the comparative method in the study of animal behavior, and his vigorous criticisms of the current dogmas regarding 'animal spirits' and the agencies and processes in muscular contraction, place him in the front rank of his contemporaries, if not, indeed, as one of the founders of physiological and comparative psychology.

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[5] John Mayow (1643–1679). *Medico-Physical Works*, 1907. (Translation of *Tractatus quinque Medico-physici*, 1674.)

[6] Robert Boyle (1627–1691). *The works of Robert Boyle*, 1744.

[7] J. du Verney (1648–1730). De la Structure et du Sentiment de la Moelle, *Mem. de l'Acad. des Sci.*, 1700.

[8] René Descartes (1596–1650). *Passions of the Soul*, 1649.

[9] Published in 1662.

[10] Foster credits Swammerdam and his contemporary, Malpighi, with being the founders of the science of Comparative Anatomy. Cf. European science in the 17th and earlier years of the 18th centuries, *Cambridge Modern History*, 1908, V, 723-741.

[11] In the introduction to his work Swammerdam indicates his interest in comparative study of behavior in the following terms: "After an attentive examination of the nature and fabrick of the least and largest animals, I cannot but allow the less an equal, or perhaps superior degree in dignity. . . . If, while we dissect with care the larger animals, we are filled with wonder at the elegant disposition of their limbs, the inimitable order of their muscles, and the regular direction of their veins, arteries and nerves; to what an height is our astonishment raised, when we discover all these parts arranged in the least, in the same regular manner." *Op. cit.*, 1.

Swammerdam's principal contributions are collected in a curious and interesting volume entitled *The Book of Nature* (*Biblia Naturae*)[12] which was published by his countryman, Hermann Boerhaave in 1738, over fifty years after the author's death.[13] An English translation of this work appeared in London in 1758 from which the material for the present study is drawn. This work is devoted primarily to the presentation of the author's anatomical and behavior studies of bees, wasps, dragon-flies, snails, butterflies, ants, worms, etc., but contains a section devoted to the development, mating behavior and anatomy of frogs. This section contains a separate essay entitled: "Experiments on the particular motion of the muscles of the frog; which may be also, in general, applied to all the motions of the muscles in men and brutes." It is here that Swammerdam has described his fundamental experiments on neuro-muscular action.

BIOGRAPHY

The father of Swammerdam was John James Swammerdam, an apothecary, born in 1606.[14] The father was "very studious of natural history, and very well skilled in several branches of it," according to Boerhaave, and seems to have made collections of insects, fossils, etc., which attracted much attention in his native city of Amsterdam. Here, also, Jan was born on February 12, 1637. In 1651 he entered the University of Leyden where he was admitted "a candidate in physic" in 1663 and received the degree of Doctor of Physic in 1667. His interest in dissection was indicated during these years by his concern with the methods of preparation and preservation of anatomical material. He succeeded, also, "in nice contrivances to dissect and otherwise manage the minutest insects" and showed "extraordinary skill in dissecting frogs."[15]

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[12] Jan Swammerdam, *The Book of Nature; or the History of Insects: reduced to distinct Classes, confirmed by particular instances, displayed in the anatomical analysis of many species.* With Life of the Author, by Hermann Boerhaave. Translated from the Dutch and Latin original Editions by Th. Flloyd. Revised and improved by notes from Reaumur and others, by John Hill, 1758.

[13] Swammerdam was "little versed in Latin," according to the biographical preface by Boerhaave in the 1738 edition of the *Biblia Naturae*, and the original works were in Dutch. On the author's death, his original manuscripts were left to Melchisedeck Thevenot, formerly the French Minister to Genoa. Boerhaave relates the history of these papers as follows: After Thevenot's death this material was "purchased by Joubert the King's painter whose heirs afterwards sold it at the inconsiderable price of fifty French crowns to the illustrious Joseph du Verney, with whom they lay hid and disregarded for a long time. . . . At last by the assistance of the reverend Mark Guitton, and the eminent William Roell, professor of anatomy at Amsterdam both then residing in Paris, I so far succeeded, that they were purchased for me the twenty-sixth of March, 1727, at the price of one thousand five hundred French florins, and received them complete the same summer."

[14] The chief source of material is the biographical introduction to the *Biblia Naturae* entitled "The Life of John Swammerdam" by Hermann Boerhaave. See also W. A. Locy, Malpighi, Swammerdam, and Leeuwenhoek, *Pop. Sci. Mon.*, 58, 1901, 561; James Duncan, Memoir of Swammerdam, *Naturalists Library*, 34, 1843; M. Foster, *op. cit.*, 99 ff.

Swammerdam's father seems to have been wholly unsympathetic with his son's interest in anatomy. The father "threatened him severely that if he did not immediately exert his talents as a physician" and desist in his "expensive researches after insects, and his experiments of that kind, he would neither supply him with money or cloaths; and to shew he was in earnest, he immediately deprived him of the means of pursuing what he had begun."

In 1674 when he was thirty-seven years old, Swammerdam finished his treatise on bees. About this time he "conceived a distaste for worldly affairs" and came under the influence of Antonia Bourignon, the Flemish mystic and Pietist (1616–1680) who seems to have dominated him for the remainder of his life. It was she who advised him to reject the offer of the Grand Duke of Tuscany of 12,000 florins for the purchase of his entomological collections. This offer was made through Swammerdam's life long friend, Stenson, the famous physiologist, and was contingent on Swammerdam's removing to Florence where "life would be easy and agreeable" and entering the Catholic Church. But Swammerdam put these Latin temptations behind him and rejected the offer "as the greatest indignity that could be offered him." After 1675 he "renounced all thoughts of human affairs, to think of nothing but his spiritual concerns, which he imagined he could not so well promote in any other manner, as by going to confer personally with Bourignon." He disposed of his collections and retired from the world until his death which occurred on February 17, 1680 when he was but forty-three years of age.

The friendship for Stenson, the controversy with his father, and the relationship with Antonia Bourignon seem to have been the important external influences in Swammerdam's life. The preoccupation with religion and the tendency towards mysticism which marked the final years of his life, were in conflict with the objectivism of his scientific inquiries. Boerhaave records that Swammerdam possessed a "temperament of the melancholy kind" and gives some indication of his inner struggles.

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[15] Boerhaave, *op. cit.* Unless otherwise indicated all biographical references are to this work.

> "His treatise of Bees was formed amidst a thousand torments and agonies of heart and mind, and self-reproaches, natural to a mind full of devotion and piety. On one hand his genius urged him to examine the miracles of the great Creator in his natural productions, whilst on the other, the love of that same all-perfect Being deeply rooted in his heart, struggled hard to persuade him, that God alone, and not his creatures, was worthy of his researches, love and attention. The distress of mind of our author felt upon this occasion, was so severe that as soon as he had finished his book upon Bees, he put it into the hands of another, without knowing or giving himself the least concern about what might become of it."[16]

The enormous industry of Swammerdam is indicated by the magnitude of his collections as described by Boerhaave.

> "These treasures consisted chiefly of insects and anatomical preparations from human subjects. . . . Our author alone had collected near three thousand species of insects, that had no relation one to another, and had examined every one of them, and disposed them all in classes according to their real and natural characters; he had even dissected many of them with that skill and dexterity peculiar to himself, and having with unwearied dilligence traced them through every the least period of their change from the egg to the Butterfly, faithfully recording all his observations, taking care at the same time to prepare and keep by him the minute originals as incontestable vouchers of his indefatigable industry in examining them. . . . He used himself to hatch, in a manner found out by, and only known to himself, the little eggs of insects, in order to discover the obscure manner of the existence of their first rudiments, the progress of these rudiments to life."[17]

CONTRIBUTIONS TO NERVE-MUSCLE PHYSIOLOGY

The conceptions of Swammerdam's contemporaries regarding tha nature of the agencies which effect muscular action were highly speculative. Crude notions of a rarefied substance ('animal spirits') which was transmitted along the nerves and inflated the muscles were held by Van Helmont and Willis. These conceptions—inherited from Galenic physiology—were presented by even so shrewd an observer as Descartes in his treatise *On Man* which was published in 1662. Few of the phenomena of integrative behavior had been demonstrated before the 17th century; and, although the muscles and nerves were recognized as the chief instruments in this behavior, the mode of their action was mystery.

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[16] *Op. cit.*, ix. In the *Biographie Universelle*, it is recorded that on one occasion Swammerdam burned all his writings on which he could lay his hands.

[17] Boerhaave, *op. cit.*, x.

The existing ignorance and the extreme difficulties of these problems were recognized by Swammerdam. He discusses these difficulties at the beginning of his study of muscular action in the frog.[18]

> "How important and difficult it is to explain the real causes of muscular motion, is sufficiently evident from numerous experiments; which though made by very ingenious men yet have not hitherto discovered its true nature. The great utility and foundation of further knowledge, which we should acquire from that discovery, lie yet involved in the thickest clouds of obscurity" (p. 122).

He notes the numerous problems, the solution of which was essential to the complete understanding of the nature of neuromuscular action. The chief among these are concerned with the structures involved, *i.e.* the nature of the neuro-muscular junction,[19] the structure of the muscular fibers, the structure of the veins and arteries attached to the muscle, and the nature of the connective tissue in the muscle. He admits the impossibility of the immediate solution of these difficulties, but, quite properly, he undertakes the task of descriptive observation of the phenomena of muscular *response to stimulation* of the nerve.

He begins by presenting his experiments which demonstrate the essential agency of the nerve in muscular action. After observing that "whenever the nerves of living bodies are handled, there is immediately observed a considerable motion in the muscles to which they are sent," he describes his experiment with an excised frog's muscle.

> "Another very delicate and useful experiment may be made, if one of the largest muscles be separated from the thigh of a Frog, in such a manner as to

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[18] Unless otherwise noted the quotations are taken from the essay on "Experiments on the particular motion of the muscles in the Frog, etc.," which is found in Part II of the *Biblia Naturae*, 122–132.

[19] This phrase is not Swammerdam's, of course. His text runs as follows: ". . . it merits particular consideration in what manner the nerve is actually joined to the muscle; how it is constructed in the muscle; what is its course, entrance, middle distribution, and end; as also how it communicates with the moving fiber, and what effect it produces in it; also what that very subtle matter properly is which is undoubtedly conveyed to the muscle through the nerve." A truly inclusive list of questions!

> remain unhurt. For if, after this, you take hold of each tendon with your hand, and then irritate the propending nerve with scissors, or any other instrument, the muscle will recover its former motion, which it had lost. You will see that it is immediately contracted and draws together, as it were, both the hands, which hold the tendons. This I formerly (in the year 1658)[20] demonstrated to the most illustrious the now reigning Grand Duke of Tuscany, when he graciously vouchsafed to pay me a visit. This experiment may be repeated in the same muscle, as long as any part of the nerve remains unhurt; and we can thus make the muscle contract itself, as often as we please" (p. 123).

This experiment, which is illustrated with lettered drawings, is the prototype of the nerve-muscle preparation which has become a standard demonstration of the physiological laboratory. Not content with the mere demonstration of the neuro-muscular response, Swammerdam desired to know "in what degree the muscle thickens in its contraction, and how far its tendons approach toward each other." He developed a technique which closely approaches modern graphic methods of recording muscular contractions.

> "We must put the muscle into a glass tube, and run two fine needles through both its tendons, where they had been before held by the fingers; and then fix the points of those needles, neither too loose nor too firmly, in a piece of cork. If afterwards you irritate the nerves you will see the muscle drawing the heads of the needles together out of the places; and that the belly of the muscle itself becomes considerably thicker in the cavity of the glass tube, and stops up the whole tube, after expelling the air. This continues till the contraction ceases, and the needles then move back into their former places; and the belly of the muscle, parting again from the tube, affords a free passage for the air through its cavity" (p. 123).

The next problem which our investigator faces has to do with the nature of the processes in the nerve. He rejects the notion, held by his contemporaries, of a rarified substance, called 'animal spirits' on several grounds. (1) The muscle continues to respond when the nerve is stimulated long after the whole preparation is cut off from the spinal cord, although the animal spirits are presumably "dissipated, or grown weak, after many times discharging their duty; and when there is not further communication between the nerve, brain, and marrow." (2) Even after the muscle is cut and the fibers are separated from each other, they continue to move when the attached nerve is irritated. (3) The hypothetical animal spirits would have to be inconceivably fine to pass through "those very fine fibrillae, which constitute the nerves." (4) The muscle shows no increase in bulk when contracted.

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[20] It is possible that this date should be 1668. Boerhaave notes in the *Life* that the Duke of Tuscany "being then in Holland with Mr. Thevenot, in order to see the curiosities of the country, came to view those of our author. . . . On this occasion Swammerdam made some anatomical dissections of insects in the presence of that great Prince."

The last point was given experimental proof by Swammerdam. Using both the excised frog's muscle and the excised heart, he demonstrated by the plethysmographic technique that there is no increase in volume of the muscle when the attached nerve is stimulated. As Fulton points out,[21] Swammerdam's experiment probably anticipated that of Glisson by several years, although it is the latter who is usually credited with this discovery.

Swammerdam summarizes his conclusions regarding animal spirits together with his hypothesis regarding the nature of nervous action as follows:

> "Therefore, I would have it seriously considered, that it cannot be demonstrated by any experiments, that any matter of sensible or comprehensible bulk flows through the nerves into the muscles. Nor does anything else pass through the nerves to the muscles; all is a very quick kind of motion, which is indeed so rapid, that it may be properly called instantaneous. There the spirit, as it is called, or that subtile matter, which flies in an instant through the nerves into the muscles, may with the greatest propriety be compared to that most swift motion, which, when one extremity of a long beam or board is struck with the finger, runs with such velocity along the wood, that it is perceived almost at the same instant at the other end; nay, that it is further propagated through the nerves into our muscles; and thus produces various in them" (p. 124).

Swammerdam confesses his inability to explain the exact nature of the action of the nerve on the muscle. He suggests, however, a "coarse similitude" between the action of muscles and that of certain plants.

> "I might conceive it effected in the same manner with the alterations visible on handling, or touching ever so gently the parts of the Herba Impatiens, called Touch me not, or podded Ars Smart, or the Balsamita alteria of Fabius Columna; the part of which plant being extended according to the course of two or three nervous or herbaceous fibers, in consequence of any momentary irritation, most suddenly contract, and the pods burst" (p. 129).

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[21] J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement*, 1926. In the "Historical Introduction" he says: "Swammerdam's use of an excised muscle was, in addition, more convincing than Glisson's demonstration with the intact human arm; moreover, the method employed by Swammerdam is that still used in physiological class rooms."

Swammerdam next concerns himself with the question of the origin of nervous action, *i.e.* where is the "beginning of that natural irritation, stimulus or provocation of the motion, thus communicated to the muscles through the nerves?" The principle of this irritation is regarded as being in the spinal cord and all the nerves of the body. They are in a state of perpetual irritation and the muscles are in a state of continuous contraction brought about,[22] according to Swammerdam, by the "continual impulse of the arterial blood upon the marrow and nerves."

ON VOLUNTARY ACTION

Swammerdam does not indulge in metaphysical speculation and pious dogmatizing, so dear to the hearts of his contemporaries, regarding the nature of the 'soul' as the seat of reason and the source of voluntary action. He deals with the problem from a point of view which will be recognized as distinctly modern.

> "I would have it particularly observed, that I admit no essential difference between the natural and spontaneous contraction of the muscles, and that performed by the will. I consider this difference as merely accidental; but, because we move all these muscles, which we move voluntarily, in a contrary direction; that which is said to be essential in the contraction of all muscles, is a natural contraction" (p. 125).

Voluntary motion is impossible unless we "have the power of determining the natural motion of the antagonists to the contrary side." Where there are no antagonistic muscles, voluntary motion is impossible. The nature of this 'determination' gives rise to a discussion of involuntary action which is significant in the light of the concept of reflex action presented by Descartes a few years before.[23]

> "Wherever the power of the antagonist is equal, there is observed no motion of the muscles, and all things are in both in equilibrio, until there arises another determination, which, causing one muscle, to be contracted somewhat more strongly than another, at length moves our limbs. Such a determination may proceed from various causes.
>
> If, for example, a man's skin be very gently rubbed and irritated with a hair doubled several times; I often observed, that the motion of the antagonist muscles of the arms and hand was immediately determined; so that the person instantaneously, as it were, unknown to him, has put his hand to the place where he felt the titillation, and then scratched the skin until he made it red,

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[22] Borelli and others had referred to the phenomenon of tonus.

[23] *Passions of the Soul,* 1649.

imagining that it was probably occasioned by a Flea, or some other insect. But when I stopt, his hand and arm rested likewise, because the natural contraction was then equal in all the muscles. If the same experiment be made on sleeping dogs and cats, it is likewise observed, that a determinate motion is produced in the muscles which move their skin; and therefore it is pleasant to see them suddenly draw it up, pricking up their hairs, and sometimes shaking them in their sleep" (p. 125-126).

The contraction of the pupil of the eye when it is irritated by a "particle of light," and the peristalsis of the intestines "in proportion to their contents" so that their contractions "resembles that of the sea, whose waves follow and mutually press upon one another," belong to the same category of action.

All manner of motor automatisms may take place without the will "as is manifest in our walking, standing, and making use of our hands: for we certainly move our limbs almost every moment, without reflecting in the least upon what we do." These actions are explained in mechanical terms, *i.e.* without any metaphysical 'cause.'

"It is for a similar reason, that, when we find ourselves too near the fire, we retire to a greater distance from it, and put our limbs, by means of various motions, into their former posture, without attending in the least to what we do; but merely in consequence of the impression made upon us by the irritating object. From hence it appears, that we can never be truely said to move voluntarily, unless when the will is put in motion by the object; and then by its own motion produces a third; for whenever the light happens to be too strong we shut our eyes, turn our head aside, and give ourselves many other motions, as we are variously excited to them by the objects that present themselves" (p. 131).

These facts "abundantly prove" that a voluntary movement is never produced except as it is preceded by a "determining principle." Since the muscles are in a "perpetual state of contraction," the "least degree of determination" is sufficient to actuate the body in a "thousand different ways." This "determining principle" may be a thought "merely casual," a dream "presenting itself before us in the night," or some pattern of physical stimuli (the "object" in Swammerdam's terminology).

Swammerdam draws a parallel between the train of our images and the continuity of muscular motions. He says:

"Thus, with regard to external objects, we often salute a person we meet, merely because another in our company takes off his hat, or because we are affected by such external object, though we do not know who the person is we have saluted, nor so much as think of our saluting him. For this reason therefore it is plain, that as our memory is local, and is assisted by the image of one thing in passing to that of another, and so on without end, the contractions of our muscles are in like manner natural; and the muscles themselves are urged by one cause of motion to another, and from this to a third, and so on without any interruption" (p. 131).

We are, perhaps, scarcely justified in interpreting Swammerdam's "Principle of Determination" in terms of present day 'determining tendencies,' but his discussion of volition is vigorous and, on the whole, consistent with the point of view of modern psychology.

In Swammerdam's discussion of the direction of the motion in nerves, he foreshadows developments in neural physiology a hundred years later. He notes that the "motion produced in the muscle by irritating the nerve, is always propagated out of the larger into the smaller branches, and goes afterwards continually descending." The nerves issuing from the spinal cord above a stimulated point are not affected when a lower point is stimulated—the twitching of the muscles serving as an indicator. This is a partial recognition of the principle of segmentation in the spinal cord. The functioning of the sensory nerves is also discussed.

> "The nerves designed for the senses are circumstanced in a quite different manner; for in these, the sensitive motions, doubtless, tend upwards. In order to contract any muscle, it is necessary that its nerve be irritated in the region above the muscle, or at its insertion into it; since that motion never tends upwards, but always downwards" (p. 125).

We have here a vague anticipation of the nervous arc—a concept elaborated by Descartes, Whytt and Bell.

Swammerdam concludes his essay by pointing out the complexity of the factors involved in every muscular contraction.

> "It would be necessary for us to consider the atmosphere, the nature of our food, the blood, the brain, marrow, and nerves, that most subtile matter which instantaneously flows to the moving fibers, and many other things, before we could obtain a sight of the perfect and certain truth" (p. 131).

ON INSTINCT

Throughout Swammerdam's studies the comparative point of view is apparent. This is illustrated in his observations on the sex behavior in insects, the phenomena of metamorphosis and life cycles of insects, and the anatomical studies. The following quotation illustrates his mode of interpretation of insect behavior.

"From these considerations it therefore follows, that among Bees there is no wiser regulation than among domestic fowls; for they have their natural desire to sit; they make nests, and nourish their young, being compelled thereto by such laws as they cannot avoid nor suppress by any rational principle; because they are impressed on them by the eternal law of nature. These little insects are under as great necessity to perform all these actions, as the winter is to follow the summer. The only difference among several kinds is, that some execute these functions in a manner more convenient, more orderly, and more agreeable to reason than others. This appears chiefly in the Bees, and hence there is no authority for the prevalent and common opinion that the government of Bees is carried on with the scepter of prudence and judgment, under law, and with rewards and punishments; for in truth all that order which we so much, and not without reason admire, is impelled by nature, and is only designed for the propagation of their species."[24]

On the Experimental Method

Swammerdam's discussion of the nature and limits of the experimental method at the conclusion of Part II of the *Book of Nature* is striking in language and significant in its psychological implications. He points out that we cannot know 'first causes' but only appearances in nature.

"I may hence, for certain, conclude, that all the knowledge and wisdom of philosophers, consists merely in an accurate perception of these elegant appearances or effects. For this reason, we should strenuously endeavor to make ourselves well acquainted with these appearances, and then draw from them firm rules and principles; otherwise as I have hinted in my preface, we may easily lose our way, and stray into the paths of error, as disputing on nature, which is quite inexhaustible, without sufficient experiments, which, on such an occasion, are as necessary to find us a path, considering with what darkness of ignorance we are surrounded, as a staff to the blind."[25]

Reason, alone, is an inadequate guide to truth. Reasoning which does not terminate in experiment is "idle and vain," whatever "syllogisms and enumerations people may think proper to build them upon." Reasoning follows observation, but the individual may be said only to have true knowledge when he can *reproduce* what he has observed.

"I believe we shall find it not unjust to dignify with the name of reason that faculty of our minds, by the assistance of which, we form clear and distinct notions of things, whilst we make proper use of our senses in sufficient experiments, so as to be able afterwards to effect exact copies of the originals we have thus endeavoured to be thoroughly acquainted with. For this reason it is, that our idea of any thing is said to be more or less clear and distinct, according as we can produce another thing more or less like it; and are therefore said to have more or less the power of it. This being granted, it should follow, that we have no clear, distinct or perfect knowledge of any things, except of such as we can truly and effectually produce."[26]

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[24] *Book of Nature*, Part I, p. 170.
[25] *Op. cit.*, p. 135-6.

By making the proper use of our senses, we may "from the things we see, gather sufficient information concerning those that we cannot."

SUMMARY

Swammerdam reflected the spirit of his time in that he regarded the body of man as a machine, and he was filled with an intense enthusiasm to exemplify the findings of physics and mechanics in its structure and functioning. Boerhaave states that Swammerdam often "wished for a year of perpetual heat and light to perfect his inquiries, with a polar night to reap all the advantages of them by proper drawings and descriptions." It is, perhaps, no small loss to science that such energy was submerged in the struggle with the 'practical' minded father and found eventual outlet in the sterile fanaticism of religion. From the point of view of physiological psychology, Swammerdam's most important contributions are in the field of neuro-mucular mechanisms in relation to voluntary and involuntary movement. These may be summarized as follows.[27]

(1) The essential agency of the nerve in muscular contraction received careful experimental verification. That the stimulation of the nerve causes the muscle to contract, was not, of course, an original discovery of Swammerdam's. This investigator, however, introduced a quantitative and graphic method for the study of the response of the excised nerve-muscle preparation.

(2) Swammerdam showed experimentally the inadequacy of the animal spirits hypothesis.

(3) He was probably the first to show experimentally, using an excised muscle, that there is no increase in bulk during the muscle contraction.

(4) He discussed voluntary action from an objective and comparative point of view rather than as a problem in meta-

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[26] *Ibid.*, 135-6.

[27] No attempt has been made in this paper to evaluate the notable contributions of Swammerdam to entomology and general physiology. He was, for example, the first to describe the red blood corpuscles, the valves of the lymphatics, etc.

physics. This is remarkable in view of the contemporary discussions and in view of his own later preoccupation with religious mysticism.

(5) He attempted to bring both the phenomena of voluntary and involuntary action under a single neuro-muscular principle.

(6) He suggested the importance of the spinal cord in neuromuscular action, especially in connection with segmentation. Implied in this there was a rather vague idea of the nervous arc concept.

THE GRAPHIC METHOD IN PSYCHOPHYSICS

By W. N. Kellogg, Columbia University

Although it might be generally assumed that an extended application of the graphic method to psychophysical judgments would prove unwieldy and valueless, the writer, who has recently had occasion to work out a technique for obtaining thresholds from kymograph records has found, on the contrary, that it is not only workable but in addition surprisingly effective in the elimination of many common sources of error. To our knowledge, the only previous attempt to record psychophysical judgments graphically appearing in the literature is reported in a study by Stephanovitch done under Wirth in 1912.[1] This investigator obtained graphic readings for Average Error adjustments but did not get a complete register of judgments for the Method of Constant Stimuli or the Method of Minimal Change. Moreover, the mechanism used in procuring his tracings was quite elaborate while that here described is simpler and permits the taking of readings directly from the shellacked records without the use of conversion formulae. Provision is also made in the present technique for its extension to any of the psychophysical methods.

The method was originally devised for the determination of differential thresholds for light intensity on a photometer,[2] but it seems to be readily adaptable to any apparatus which can be arranged for reading judgments on a linear scale. Galton Bar records obtained from comparisons of lengths of lines or sound intensity thresholds read from the scale attached to a coil rheostat are examples of possible applications to standard apparatus.[3] Even the classical lifted weight experiment with slight modifications offers possibilities for graphic recording.

Method

Manner of obtaining curves. Our records were obtained with the use of an ordinary Stoelting kymograph, operated horizontally. The principal writing point, which slid from side to side across the kymograph drum, was fastened by means of a horizontal extension arm to the movable lamp of the photometer which produced the different intensities of the variable stimulus.[4] Two electromagnetic writing points recorded judgments from the *S*'s right and left hand keys respectively, and a time marker, registering in sec. or 1/5 sec., traced the time record. One additional electromagnetic pointer was added to

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[1] J. Stephanovitch, Untersuchung der Herstellung der subjektiven Gleichheit bei der Methode der mittlere Fehler, unter Anwendung der Registriermethode, *Psychol. Stud.* 8, 1912, 77-116.

[2] In connection with an investigation under the direction of Professor R. S. Woodworth, who made the original suggestion regarding a graphic application.

[3] H. M. Halverson (Tonal volume as a function of intensity, this Journal, 35, 1924, 360-367) and E. G. Wever and S. R. Truman (The course of the auditory threshold in the presence of a tonal background, *J. Exper. Psychol.*, 11, 1928, 98-112) obtained sound intensity thresholds in terms of such a scale.

[4] In using a Galton Bar the extension arm would attach to the scale pointer (fastened to the movable part of the bar) which normally records Galton Bar readings. With a rheostat scale, the connection could be made to the sliding contact point which alters the intensities of the sounds.

Procedure. The method of paired comparisons was again employed. Every stimulus-combinations was compared with itself and with every other stimulus-combination in the two space orders. Thus 100 comparisons were given by every *O*. When the systems of each grill had been brought to the desired temperatures, the *Os* were called in separately. Seated before the grills, *O*, at a signal from *E*, placed his forearms upon them, volar surfaces down, and compared the intensities of the heat sensations aroused. No time limit was imposed; as a general rule, however, all the *Os* found the judgments easy and returned them quickly.

When all the *Os* had given their judgments upon a particular pair of stimulus-combinations, *E* interchanged the grills, thus altering the space order, and the *Os* were again called in. This procedure was repeated throughout the course of the experiment.

Instructions. The following instructions were given the *Os* at the beginning of every experimental hour.

"At the signal 'now,' place the volar surfaces of your forearms upon the grills before you. Relax and keep the pressure of your arms as constant as possible. Report which of the two grills, if either, is the hotter. If they are equally hot, report 'equal;' if only one is hot, designate it; if neither is hot, report that fact."

Observers. The *Os* were Miss Estelle Doniger (D), Miss Eleanor Lowenstein (L), and Mr. John S. Riedel (R), all senior undergraduates majoring in psychology. They were naïve in the observation of heat and they served without knowledge of the problem other than that given them in the instructions.

RESULTS

The accompanying tables show the results of the study. Table I gives the crude data. The boldface diagonals give the results of the *Os*' judgments when the various stimulus-combinations were compared with themselves. The data below the diagonals show the stimulus-combinations that were judged 'hotter' in the first space order, *i.e.* Grill I at the left and Grill II at the right; the data above the diagonals, the results in the second, reverse space order. The number of the combination judged the 'hotter' in every comparison is set down in the table; 'no' represents the experiments in which no heat was experienced.

An examination of Table I shows that the judgments were not materially affected by space order. D reported the more intense heat 43.5 times at the left and 40.5 times at the right;[4] L, 38.5 and 45.5 times; and R, 40 and 46 times. The differences between the numbers are small (−3, +7, and +6 respectively for D, L, and R) and are, we believe, due to chance. The results of the comparisons of the various stimulus-combinations with themselves confirm in general this belief. In the 10 comparisons of identical stimulus-combinations, D reported the left as the more intense once and the right twice; L, the right and left 3 times each; and R, the left once and the right 5 times. R may have been slightly disposed toward the right, but there is certainly little indication of a space error in the results of D and L.

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[4] Equal judgments were counted as 0.5 for each side.

TABLE I

CRUDE DATA, SHOWING FOR EVERY COMPARISON THE STIMULUS-COMBINATION THAT WAS JUDGED 'HOTTER'

Boldface diagonals give the results when the various stimulus-combinations were compared with themselves; data below diagonals give the results of the comparisons in the first space order, above diagonal the results of the comparisons in the second space order

O		Stimulus-temperatures		Grill II									
				12°+43°	17°+43°	13°+40°	18°+40°	14°+37°	19°+37°	24°+37°	20°+34°	25°+34°	30°+34°
			No.	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*
D	Grill I	12°+43°	*1*	**=**	*1*	*3*	*1*	*1*	*1*	*1*	*1*	*1*	*1*
		17°+43°	*2*	*1*	**=**	*2*	*2*	*2*	*2*	*2*	*2*	*2*	*2*
		13°+40°	*3*	*1*	*2*	**R**	*3*	*3*	*3*	*3*	*3*	*3*	*3*
		18°+40°	*4*	*1*	*2*	*3*	**=**	*4*	*6*	*4*	*4*	*4*	*4*
		14°+37°	*5*	*1*	*2*	*3*	*4*	**R**	*5*	*7*	*5*	*5*	*5*
		19°+37°	*6*	*1*	*2*	*3*	*4*	*6*	**=**	=	*6*	*6*	*6*
		24°+37°	*7*	*1*	*2*	*3*	*4*	*7*	*7*	**L**	*8*	*9*	*7*
		20°+34°	*8*	*1*	*2*	*3*	*4*	*8*	*6*	*8*	**no**	no	no
		25°+34°	*9*	*1*	*2*	*3*	*4*	*9*	*6*	*9*	no	**no**	no
		30°+34°	*10*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	no	no	**no**
L	Grill I	12°+43°	*1*	**R**	*1*	*1*	*1*	*1*	*1*	*1*	*1*	*1*	*1*
		17°+43°	*2*	*2*	**R**	*2*	*2*	=	*2*	*2*	*2*	*2*	*2*
		13°+40°	*3*	*1*	*2*	**L**	*3*	*5*	=	*3*	*3*	*3*	*3*
		18°+40°	*4*	*1*	*2*	*3*	**=**	*4*	*6*	*4*	*4*	*4*	*4*
		14°+37°	*5*	*1*	=	*3*	*4*	**R**	*6*	*7*	*8*	*9*	*10*
		19°+37°	*6*	*1*	*2*	*3*	*4*	*6*	**L**	*7*	*6*	*6*	*10*
		24°+37°	*7*	*1*	*2*	*3*	*4*	*7*	*7*	**L**	=	*7*	*7*
		20°+34°	*8*	*1*	*2*	*3*	*4*	*8*	*6*	=	**no**	no	no
		25°+34°	*9*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	no	**no**	no
		30°+34°	*10*	*1*	*2*	*3*	*4*	*5*	*6*	*10*	no	no	**no**
R	Grill I	12°+43°	*1*	**R**	*1*	*1*	=	*1*	*1*	*1*	*1*	*1*	*1*
		17°+43°	*2*	*1*	**R**	*2*	*2*	*2*	*2*	*2*	*2*	*2*	*2*
		13°+40°	*3*	*1*	*2*	**=**	*3*	*3*	*3*	*3*	*3*	*3*	*3*
		18°+40°	*4*	*1*	=	*3*	**R**	*4*	*4*	*4*	*4*	*4*	*4*
		14°+37°	*5*	*1*	*2*	*3*	*4*	**R**	*5*	*7*	*8*	*5*	*10*
		19°+37°	*6*	*1*	*2*	*3*	*4*	*5*	**R**	*7*	*8*	*6*	*6*
		24°+37°	*7*	*1*	*2*	*3*	*4*	*7*	*7*	**L**	*8*	*9*	*7*
		20°+34°	*8*	*1*	*2*	*3*	*4*	*8*	*8*	*8*	**no**	no	*8*
		25°+34°	*9*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	no	**no**	no
		30°+34°	*10*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	no	**no**

Table II, comparable to Table VI in the earlier study,[5] shows the data in the two space orders, arranged and classified according to the size of the stimulus-differentials used in this experiment. The differential values appear in the table in descending rank order under the caption 'Stimulus-differential.' Opposite these values, in the columns to the left, appear the stimulus-temperatures that were used to produce them; and in the columns to the right appear the number of times that 'hotter' reports were returned by every *O* for the different temperature-combinations, together with the rank orders of the temperature-combinations as determined by these reports. An examination of the rank orders shows that they follow very closely the rank order determined by the stimulus-differential as computed by Burnett and Dallenbach's formula. The correlations, as calculated by the method of rank differences,[6] are 0.960 for D, 0.943 for L, and 0.897 for R. These correlations are high (surprisingly so when it is recalled that our *O*s were relatively untrained in observation), and they indicate that the intensity of the sensation of heat aroused by warm and cold stimuli varies, also under our conditions, according to the formula $h \propto (32 - c) + 3(w - 33)$.[7]

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[5] *Op. cit.*, 493.

TABLE II

SHOWING THE STIMULUS-COMBINATIONS USED IN THE EXPERIMENT, THE STIMULUS-DIFFERENTIALS CALCULATED BY THE PROPOSED FORMULA ARRANGED IN RANK ORDER, AND THE CORRELATION OF THE *O*S REPORTS WITH THEM

Stimulus-				Observers					
				D		L		R	
Temperatures c w	No.	Stimulus-differential $(32-c)+3(w-33)$	Rank order	Times 'hotter'	Rank order	Times 'hotter'	Rank order	Times 'hotter'	Rank order
12°+43°	1	50	1	17	1	17	1	17.5	1
17°+43°	2	45	2	16	2	16	2	15.5	2
13°+40°	3	40	3	15	3	12.5	3	14	3
18°+40°	4	35	4	11	4	11	4	13	4
14°+37°	5	30	5	5	7	4	7	5	7
19°+37°	6	25	6	8.5	5	8.5	5	4	8
24°+37°	7	20	7	5.5	6	8	6	7	5.5
20°+34°	8	15	8	3	8.5	3	8.5	7	5.5
25°+34°	9	10	9	3	8.5	1	10	1	9.5
30°+34°	10	5	10	0	10	3	8.5	1	9.5
			r =	0.960		0.943		0.897	

This conclusion receives further confirmation from the fact that the rank orders of the *O*s' reports correlate more closely with the rank order of the stimulus-differentials than they do with each other. D's and L's reports give a rank order correlation of 0.972; L's and R's, a correlation of 0.882; and D's and R's, a correlation also of 0.882. The average correlation among the *O*s is 0.912. The average correlation of the *O*s' reports with the stimulus-differentials is 0.933. The difference between these averages is not great—and we should not have expected it to be great, since all the *Os* were judging the same scale of differential values—but the fact that it lies in a direction favorable to the correlations with the stimulus-differentials indicates that the stimulus-differentials and not some other common factor determined their reports. Had the result been otherwise, *i.e.* had the *Os'* reports correlated more closely among themselves than they did with the rank order of the stimulus-differentials, we should have suspected the presence of other factors. Similar results were obtained in the earlier study, though the authors did not compute or present them. The average correlation of the reports of the *Os* among themselves is, in that study, 0.882; whereas the average correlation of their reports with the stimulus-differentials, as those values were computed by this formula, is 0.895. The difference again is small, but again it lies in the direction favorable to correlations with the stimulus-differentials.

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[6] $r = 1 - [6\Sigma D^2/n(n^2 - 1)]$.

[7] This formula is valid, however, only when and where the physiological zero or zone of indifference falls between 32° − 33°C. When the zone of indifference has other limiting values, those values should be substituted. Specific values should not be set to the zone of indifference if the formula is to have general validity. A generalized formula would read: $h \propto (x - c) + 3(w - y)$, where x and y represent respectively the lower and upper limits of the zone of indifference. Whether this generalized formula will hold, future experiments can alone determine.

SUMMARY

In order to test the formula proposed by Burnett and Dallenbach for the intensive gradations of heat aroused by warm and cold temperatures, we selected 10 temperature-combinations that gave a uniform and regular series of differential-values, as calculated by the formula, and compared the intensity of the resulting heat sensations by the method of paired comparisons.

We found (1) that the rank order of the temperature-combinations, as determined by the number of 'hotter' reports returned by the different *Os*, correlated very closely with the rank order of the differential values; and (2) that the rank orders of the *Os'* reports correlated more closely with the rank order of the differential values than they did with each other. We feel justified, therefore, in stating that the formula $h \propto (32-c) + 3(w-33)$ is adequate under our conditions to the computation of the intensive gradations of heat aroused by warm and cold stimuli.

LXXV. DISTANCE FROM POINT OF FIXATION *VS.* INTENSITY AS A DETERMINANT OF ATTENTION

By C. L. FRIEDLINE and KARL M. DALLENBACH

In the series of studies on the determinants of attention that have been published from the Cornell Laboratory,[1] the stimulus-objects—visual areas of

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* Accepted for publication February 5, 1929.

[1] L. G. Meads, Form *vs.* intensity as a determinant of attention, this JOURNAL, 26, 1915, 150-151.

J. N. Curtis and W. S. Foster, Size *vs.* intensity as a determinant of attention, *ibid.*, 28, 1917, 293-296.

A. M. Bowman, Size *vs.* intensity as a determinant of attention, *ibid.*, 31, 1920, 87-90.

K. M. Dallenbach, Position *vs.* intensity as a determinant of clearness, *ibid.*, 34, 1923, 282-286.

Doris Dewey and K. M. Dallenbach, Size *vs.* intensity as a determinant of attention, *ibid.*, 35, 1924, 121-125.

R. S. Burke and K. M. Dallenbach, Position *vs.* intensity as a determinant of attention of left-handed observers, *ibid.*, 35, 1924, 267-269.

standard and variable illumination—have been placed at equal distances from the point of fixation. Though the authors of these studies did not know definitely what effect, if any, unequal distances would have upon attention, they kept their stimulus-objects at equal and constant distances from the fixation-point as a matter of sound technique. The problem that they thus ignored, or rather avoided, was taken as the subject of this study. We wished to know definitely whether distance from point of fixation was a determinant of attention; whether, to be specific, a stimulus-object near the point of visual fixation was more or less attention compelling than one further removed.

METHOD

We employed the method used in the earlier studies. Two stimulus-objects, alike in every respect except distance from point of fixation and intensity of illumination, were compared for their attentional value by determining the objective intensity at which they were reported equally attense (clear, vivid).

Apparatus and procedure. We used the exposure apparatus that had been developed in the Cornell laboratory for the study and equation of the conditions of attention.[2] The *O* sat in a dark room in a light-proof booth, with his head held firmly by a biting board which was so adjusted that the fixation-point, 1 m. away and 1 mm. in diam., would fall, if extended, between his eyes. Circular stimulus-areas, 3 cm. in diam., were located on the horizontal axis on a black field and on opposite sides of the fixation-point at distances, as measured from the inner edges of the areas, of 11 cm. and 17 cm. respectively. The length of the exposures was, as in the earlier studies, approximately 0.1 sec.

One of the areas, the standard, was kept at a constant intensity, *i.e.* at 16% of the light that could, within the limits of the apparatus, fall upon it. The intensity of the other area, the variable, was varied according to the requirements of the experiment.

The position of the area of standard intensity was varied—in haphazard order—from near to far, and also, because of the large space error involved in these experiments,[3] from right to left. In Series 1 the area of standard intensity was 11 cm. to the right of the fixation-point and the area of variable intensity was 17 cm. to the left; in Series 2 the standard intensity was 11 cm. to the left and the variable was 17 cm. to the right; in Series 3 the standard was 17 cm. to the left and the variable 11 cm. to the right; and in Series 4 the standard was 17 cm. to the right and the variable 11 cm. to the left (cf. Fig. 1).

After a preliminary series of experiments by the method of limits, which were made in order to determine the stimulus-intensities required for the variable by every *O* in every one of the 4 series of experiments, the main experiments were performed by the method of constant stimulus-differences.

Instructions. The following instructions were read to the *O*s at the beginning of every experimental hour.

"At the signal 'Ready' fixate and attend to the spot of light before you. At 'Now' two circular areas, one on each side of the fixation-point, will be exposed. Report which, if either, is the more clear. Give the position of the clearer as 'right' or 'left,' whichever the case may be; or, if they are equally clear, report 'same.' If you are uncertain say 'doubtful.' "

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[2] K. M. Dallenbach, An apparatus for the study of the conditions of clearness, this JOURNAL, 34, 1923, 94-95.

[3] K. M. Dallenbach, Position *vs.* intensity as a determinant of clearness, *ibid.*, 34, 1923, 282.

The experiments in which 'doubtful' was reported were, without *O*'s knowledge, immediately repeated.

At the beginning of every observational period a 'warming-up' series of 10 experiments was given the *O*s. They did not know, however, when this series ended and the regular series began. The *O*s were allowed frequent rests during the series, but we were nevertheless able to obtain 10 series of from 5 to 7 experiments each—depending upon the number of variable stimuli used—during every period. We planned 50 series for every one of the 4 positions of the standard area.

Observers. We used 3 *O*s, but we were unable, owing to the length of the experiment and the shortness of the term,[4] to complete the experiments with two of them.[5] The *O*, Miss Ruth Burke, who completed the experiments, was trained in the observation of attensity and familar with the work.[6]

RESULTS

The results for the *O* who completed the work are shown in Table I, which gives for every one of the four series of experiments the percentage of illumination necessary for the variable area to be reported equal to the standard in attensity (clearness, vividness). The results are also shown diagrammatically in Fig. 1.

TABLE I

SHOWING THE PERCENTAGE OF ILLUMINATION NECESSARY FOR THE VARIABLE TO EQUAL THE STANDARD IN ATTENSITY

(STANDARD AT 16%)

Series	Relation of Variable to Fixation-Point		Percentage of Illumination (L)	Precision (h)
	Position	Distance		
1	left	17 cm.	21.58	0.05
2	right	17 cm.	28.21	0.03
3	right	11 cm.	10.25	0.13
4	left	11 cm.	9.79	0.12

The data show that the area 11 cm. away from the point of fixation requires less illumination than the area 17 cm. away in order to be judged equal to it in attensity. This relation obtains, as shown in the second and third columns of Table II, in every one of the four series of experiments as well as in the averages for the entire experiment.

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[4] This study was made during a summer term of six weeks.

[5] These *O*s were untrained and inexperienced, consequently a short practice series in the observation of attensity had to be given them. This delayed us, but we should easily have completed the required number of experiments had not illness and the pressure of the term examinations made it necessary to excuse them.

[6] This *O* had previously conducted a similar experiment; cf. footnote 1 above.

The differences are large and significant, and they warrant the conclusion, in so far as we may be justified in drawing conclusions from the results of one *O*, that distance from point of fixation is a condition of visual attention. The stimulus-object close to the point of fixation has, by virtue of its nearness alone, an attentional advantage over a stimulus-object further away.[7] The near object need not be as intense as the far object in order to equal it (the far object) in attensity.

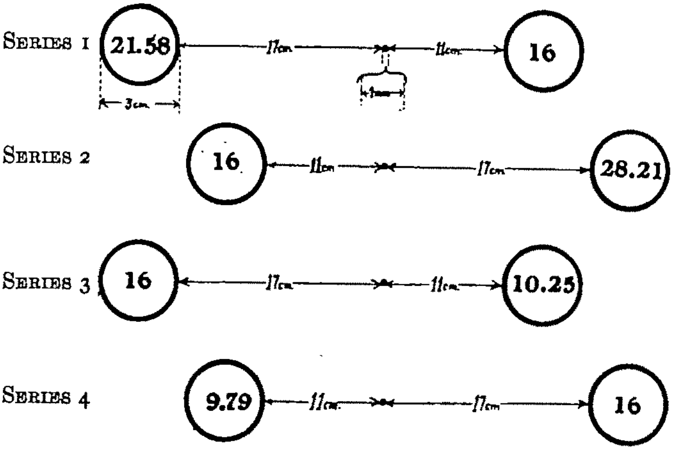

FIG. 1. SHOWING THE PERCENTAGES OF ILLUMINATION AT THE DIFFERENT DISTANCES AND POSITIONS THAT WERE JUDGED EQUALLY ATTENSE

TABLE II

SHOWING THE PERCENTAGES OF ILLUMINATION FOR THE DIFFERENT DISTANCES AND DIFFERENT POSITIONS THAT WERE JUDGED EQUALLY ATTENSE

Series	Distance of Stimulus-Area from Fixation-Point		Position of Stimulus-Area from Fixation-Point	
	11 cm.	17 cm.	Left	Right
1	16.00%	21.58%	21.58%	16.00%
2	16.00%	28.21%	16.00%	28.21%
3	10.25%	16.00%	16.00%	10.25%
4	9.79%	16.00%	9.79%	16.00%
Av.	13.01%	20.45%	15.84%	17.62%

The data also show that the position to the left of the fixation-point has an attentional advantage, thus corroborating the results of Dallenbach.[8] The stimulus-area to the left, as shown by the averages in the last two columns of Table II, requires less illumination than the area to the right in order to be judged equally attense. The effect of position is obscured in the results of the different series by the effect of distance from fixation, but it becomes evident when distance is cancelled out by averaging. The position to the left requires an illumination of 15.84%, that to the right an illumination of 17.62%

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[7] This is probably due to differences in retinal sensitivity. We were not, however, interested in this study in determining the cause of the difference, we wished merely to determine whether there was a difference.

[8] Dallenbach, *op. cit.*, 285 f.

The fact that the effect of position is obscured in the results of the different series, and that it shows itself only when the effect of distance from point of fixation is averaged out, indicates that 'distance' is, under our conditions, a stronger determinant of attention than 'position.'

Summary. The results of this study show: (1) that distance from point of fixation is a determinant of attention; (2) that stimulus-objects near the point of fixation have an attentional advantage over objects further removed; and (3), in corroboration of earlier results, that position is also a determinant of attention.

APPARATUS

REACTION-KEYS AND A NEW TECHNIQUE FOR READING-REACTIONS

By SIEGEN K. CHOU, Stanford University

Titchener listed no fewer than 20 varieties of reaction-keys of all types depending upon the mode of the reacting movement.[1] Of voice-keys there are Cattell's, Roemer's, Wundt's, and Libby-Baldwin's; of eye-keys there are the eyelid-key listed by Zimmermann and the Dodge pendulum reaction-key; Cattell's lip-key, Meumann's biting-key, Kraepelin's speech-key, and Jastrow's speech-key are all adapted for vocal reactions; and Moldenhauer's smell-stimulator is akin to a reaction-key. By far the most extensive reaction-movement and that which yielded the greatest number of reaction-keys is the finger movement. The key ordinarily employed for this mode of reaction is the telegraph-key. Wundt had the early modified form; Zimmerman listed a simple form; Jastrow described the press-key; Ranschburg's key, Ewald's rocker, and Scripture's multiple key permitted double or multiple contacts and instantaneous make and break. All these types make use of one finger only. Dessoir introduced in 1892 the thumb-and-finger key.[2] "In its original form, it consisted of two hard rubber rings, fitted with contacts and binding posts, and shaped to slip over *O*'s thumb and finger; the range of movement was limited by two inelastic bands fastened to the rings on either side."[3] Scripture modified Dessoir's key and described his modification in several places.[4]

Trace described "a new tapping instrument" which was developed in the Stanford laboratory after "about a year's experimentation."[5] In describing the apparatus Trace says, "The instrument is light and small, . . . can be raised or lowered to meet the needs of the reagents; it can be used in either the horizontal or the vertical position."[6] Trace evidently was not aware of Titchener's comment, "Dessoir remarks that his thumb-and-finger key 'in jeder Beziehung sehr bequem ist.' "[7] She seemed to be unaware also of Scripture's modification. Noticing that the rate of the thumb-and-finger key is much slower than the tapping board and key, so that changes in rate and character of tapping and fatigue may be easily noted, Trace pointed out that "this introspective phase is a valuable addition to the tapping experiment."[8] Scripture wrote in 1893 of his modification of Dessoir's thumb-and-finger key: "While using this key in an exercise on the rapidity of tapping movements of the finger we found that we had the means at hand of making new investigations on the time and extent of voluntary movement. All previous work in recording taps had been done with an ordinary telegraph-key by which only the moment of the extremity of the downward movement was recorded. By simply connecting the binding-posts of the cross-piece and the fixed slide to one end of the circuit and the post of the movable slide to another, a record was made of the downward extreme, the upward extreme and the period of rest at each extreme. Moreover, the adjustment of the fixed slide gave any desired extent for movement."[9]

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[1] E. B. Titchener, *Experimental Psychology, Quantitative Experiments, Instructor's Manual*, 1905, 349-351.

[2] M. Dessoir, Über den Hautsinn, *Arch. f. Physiol.*, 1892, 311.

[3] Titchener, *op. cit.*, 349.

[4] E. W. Scripture, *Stud. Yale Psychol. Lab.*, 1, 1893, 88; *Thinking, Feeling, Doing*, 1895, 43-44; *The New Psychology*, 1898, 121-122.

[5] G. M. Trace, A new tapping instrument for laboratory use, this JOURNAL, 30, 1919, 425-426.

[6] *Ibid.*, 425.

[7] Titchener, *op. cit.*, 355.

[8] Trace, *op. cit.*, 426.

Trace did not say that her statement that the rate of the thumb-and-finger key is much slower than the rate of the tapping board and the telegraph-key was based on direct experimental comparison. The writer made a direct comparison at the suggestion of Dr. Franklin Fearing in the summer of 1926 between the two rates of tapping on the ordinary telegraph key and the Stanford thumb-and-finger key. A pendulum time-marker was first calibrated and then used to time the two modes of tapping with the ordinary kymographic technique. The right index finger was used in the ordinary key tapping and the thumb and index finger were used in the thumb-and-finger key tapping with the key in the vertical position. The writer obtained on himself 40 series of 30 sec. each in 5-sec. intervals (Wells' series) for each of the two modes of tapping. The following table shows the result:

TABLE I

SHOWING THE RESULTS OF THE TWO MODES OF TAPPING

Key	Series	Intervals of 5 sec.					
		1st	2nd	3rd	4th	5th	6th
Telegraph	1st 20	20.3	19.3	18.7	17.2	17.3	16.9
	2nd 20	22.9	23.1	22.3	22.9	21.7	21.5
	Av.	21.6	21.2	20.5	19.6	19.5	19.2
Thumb-and-finger	1st 20	23.5	23.7	22.4	21.3	21.4	19.6
	2nd 20	24.1	23.2	22.0	21.8	21.4	20.2
	Av.	23.8	23.5	22.2	21.5	21.4	19.9

Contrary to Trace's casual mention, my direct comparison definitely shows that the rate of tapping is decidedly faster in the thumb-and-finger key than in the ordinary telegraph key, as is evident from the means of the two experiments of 20 series each. This is even more apparent in the first experiment of 20 series, although in the second experiment the rate of tapping in the thumb-and-finger key drops below that of the telegraph key from the third interval on. This is, however, in accord with Trace's contention that fatigue is more easily noted in the thumb-and-finger key. Although the fatigue effect during the first experiment of 20 series was practically the same in both keys, it drops decidedly during the second experiment in the telegraph key; so that the average fatigue-index in terms of percentage loss of the number of taps in the last interval over the first is much less. The average fatigue-index for the thumb-and-finger key is 16.40%, while that for the telegraph key is 11.44%. In other words, the telegraph-key single-finger reaction is much more susceptible to improvement than the double thumb-and-finger reaction. The double-finger reaction is much less variable. My reports also show that in the one-finger reaction much more intense kinesthesis was felt, and that it required far greater effort as the tapping went on. In the case of the two-finger reaction, however, there was almost nothing extraordinary, the fingers coming together and pulling apart as a matter of course.

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[9] E. W. Scripture and J. M. Moore, A new reaction-key and the time of voluntary movement, *Stud. Yale Psychol. Lab.*, 1, 1893, 89-90.

In discussing the reaction-movement, Titchener remarked: "Practically all those who have worked with the lip-key have noted a tendency to premature reactions; the lips are often opened before the reaction proper is made."[10] The writer noted a like tendency to premature reactions even in the ordinary telegraph key employed in his new Quadrant Tachistoscope[11] in connection with a series of investigations on the reading of Chinese characters.[12] The *S* is required to press the key down to open the quadrant shutters of the tachistoscope either vertically or horizontally for the two corresponding arrangements of reading material, and to hold it there as long as he is reading, but to lift his finger as soon as he is through reading, the reading time being marked by a stop-watch. Although most of the *S*s adjusted perfectly to this new type of reading-reaction, there were a few who seemed to be troubled by it because not infrequently they lifted or rather slipped their finger off the key and hence closed the window thus destroying their view. While perhaps the antagonistic downward pressure which Titchener reported in all muscular reactions[13] may be present when *S* is about to lift his finger to close the window after reading (although there is no such introspective report from any *S*), the premature lifting reaction from the key is not quite comparable with the premature reaction in the lip-key, because here a much more complex perceptual reading-reaction is involved. Since this method of requiring *S* to hold down the key as long as he is reading (in order to open the shutters of the tachistoscope exposing the reading material) is a new approach in the technique of recording reading-reactions, a word or two about its theoretical nature may be offered here.

The usual method of recording the speed or rate of reading is to obtain the total time in reading a passage and then to find the average rate in terms of words read per sec. by dividing the total number of words by the total time. The efficiency of reading is thus noted in terms of amount done in unit time, rather than of time required for reading a unit amount of material. The departure of the method adopted by the writer in his series of investigations on the reading and legibility of Chinese characters by means of his new quadrant tachistoscope exactly reverses the ordinary procedure, by taking sentences of seven characters as a unit amount of material to be read and recording the reading time for every unit. The theoretical justifications for this procedure may be mentioned.

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[10] Titchener, *op. cit.*, 355.

[11] S. K. Chou, A quadrant tachistoscope for studying the legibility of Chinese characters, *J. Exper. Psychol.*, 12, 1929, 178-186.

[12] S. K. Chou, Reading and legibility of Chinese characters. I. The influence of reading-direction and character-position upon speed of reading, *ibid.*, 156-177.

[13] E. B. Titchener, *op. cit.*, 351. Cf. R. D. Williams, Experimental analysis of forms of reaction-movement, *Psychol. Monog.*, 17, 1914, (no. 75), 102, 128, 136, 148.

In the first place, as Woodrow pointed out when he used reaction-time as the measure of degree of attention, a small amount in a given time does not allow enough room, so to speak, to let the lower extreme of ability have free play. "Efficiency can not be adequately measured by the amount done in a given time. Paradoxical as it may sound, the time required to do a given amount is a much more adequate and accurate measure than the amount done in a given time. In measurement by amount done, poor efficiency is not given sufficient chance to display itself. If a person is rather poor and so accomplishes only a small amount in a given time, there is, so to speak, lack of room for a great absolute decrease in this amount done, no matter how much worse the individual becomes. There is no limit on the other hand to the increase in time that he may require to accomplish a given amount of work. There is infinite room for the time to become longer."[14]

In the second place, analogous to Woodrow's general law of detraction, the effect of factors such as reading-direction and character-position upon the speed of reading Chinese cannot be ascertained if efficiency be measured by words read per sec. For example,[15] the fastest reader (9W) read on the average 5.36 words per sec. under the most favorable combination of leftward reading of upright characters, and 4.24 under the least favorable combination of rightward reading of upside-down characters; the slowest reader (1W) read on the average 2.11 and 1.87 respectively. If we take the absolute decrease of number of words read per sec. as the detrimental effect of a certain combination of reading direction and character-position, then the slowest reader would be less affected, as his decrease is 0.24 words, while that of the fastest reader is 1.12 words. As a matter of fact, reader 9W took 1.306 sec. to read the seven characters under the most favorable combination and 1.650 sec. under the least; while reader 1W took 3.312 and 3.738 respectively. If now we take the increase in reading time as a measure of the influence of a certain combination of reading-direction and character-position, the slowest reader would be much more affected, as his increase in time is 0.426 sec., while that of the fastest reader is only 0.344 sec.

In the third place, the taking of the reading reaction-time as a unit amount for measuring the speed of reading has the further advantage of securing a large number of reaction-measures for statistical treatment in evaluating the various effects of various factors which influence the speed or efficiency of reading Chinese. The effect of such factors as reading-direction, character-position, temporal-spatial sequence and half-character on the efficiency of reading, cannot be adequately determined by recording the total time for reading a passage and then artificially breaking the passage into words read per sec., because statistically this representative measure is not sound since it is in no sense a central measure of any distribution of separate measures, but an abstraction in divisable units from a single measure of larger unit. A single time measure of any passage is more influenced by the total content than by any specific factor that is introduced in the manner of reading or in the arrangement of characters, and hence can not be taken as an index of the effect of the latter; much less when it is artificially divided into shorter time units.

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[14] H. Woodrow, The measurement of attention, *Psychol. Monog.*, 17, 1914, (no. 76), 13.

[15] S. K. Chou, Legibility of Chinese characters, influence of reading-direction and character-position upon the speed of reading Chinese by means of a new quadrant tachistoscope, Stanford Univ., Unpubl. Diss., June, 1928.

Returning to the premature lifting of the finger from the key controlling the shutters of the tachistoscope, it may be pointed out that this procedure of requiring *S* to press down the key while he is reading constitutes exactly what Woodrow[16] called the detraction method. He distinguishes the detraction method from the ordinary distraction method by the fact that a distractor unavoidably implies the idea of division or tearing asunder of attention, while the detractor simply lowers the degree of attention. Reading with the finger pressing down the key all the time so as to keep the window shutters open is certainly lowering the degree of attention, since this pressing down of the finger is an unavoidable state of one of the conditions of attention, as any unfavorable preparatory interval is. A simultaneous pressure upon the key results in a prolongation of the reading reaction-time, just as the unfavorable preparatory intervals would, because of its effect as detractor of attention. On the other hand, there is reason to believe that it may be at the same time an intensifier of attention. The reader knows that so long as he is holding down the key he is free to read whatever is exposed in the tachistoscope, but as soon as there is the least roaming of attention his finger is liable to slip off from the key thus closing the shutters and destroying his view. This is perhaps the only interpretation of the fact that while the majority of the readers adapted to the situation perfectly without the slightest indication of being distracted by the pressing down of the finger, there were several who seemed to need extra concentration on the finger. One reader lifted his finger from the key as many as 23 times during the whole course of an experiment consisting of 128 reading-reactions, another 15, and still another 10 times. These readers are personally known to the writer as belonging to the 'nervous' type. For the more stable readers, the pressing down of the finger becomes so automatized that they are no more aware of the fact while they are reading than they are of their walking while thinking. The reader rather than the *E* is required to open the shutter in this particular technique because by so doing the fullest degree of attention is secured; since the reader would not press down the key even after the signal unless he were perfectly ready to read. The key is pressed down all the time during reading and is not to be released until the reader finishes his reading, instead of being pressed down once for opening, the shutter being allowed to stay open, or the *E* being allowed to close it, because this kind of reaction actually keeps the *S* on the alert all the time, a slight roaming of the attention being sufficient to break the circuit, thus closing the window and destroying the field of view. The simultaneous voluntary maintenance of the exposure of the tachistoscope during the reading reaction is, therefore, at least a check upon, if not an intensifier of, the attention of the observer.

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[16] Woodrow, *op. cit.*, 9.

A VACUUM TUBE FOR STABILIZING THE CURRENT DURING MEASUREMENTS OF THE GALVANIC REFLEX

By ROLAND C. DAVIS, Columbia University

In galvanometric work it is usual to compensate for large variations of current through the subject and to measure small ones. The circuit represented in Fig. 1 is intended to keep the current constant during large and small resistance variations and allow the changes in voltage drop to be measured.[1] (Voltage drop would not affect polarization if current is independent of it.) *S* is placed in the plate circuit of a vacuum tube as a convenient means of obtaining a resistance in series with him, that is high relative to his own. The filament of the tube is heated in the usual manner by a 6 v. storage battery A, acting through the filament rheostat R. The grid of the tube is kept at a negative potential by means of the battery and potentiometer C_1. Variation of this potential will, of course, determine the size of the plate current flowing through the *S*. This current is supplied from the battery B, and measured by the instrument M_S. The current can be set at a given value by means of the potentiometer C_1, and will remain fixed.

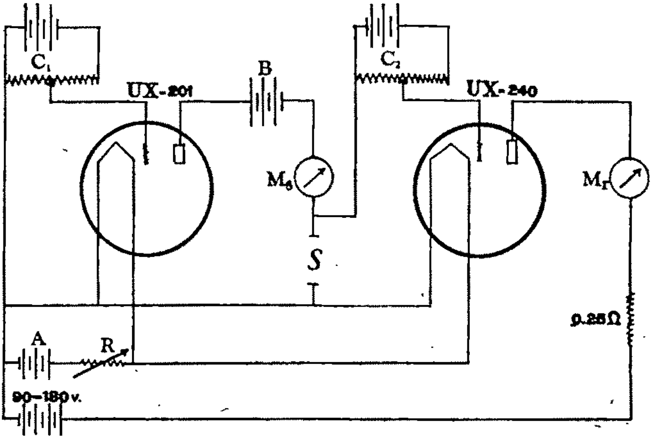

FIG. 1. GALVANOMETER CIRCUIT, WITH STABILIZING VACUUM TUBE

For instance, using a plate supply (B) of 50 v., and an available bias of 9 v. (C_1) on a UX-201 A tube, the plate current can be set at 0.05 milliampere. It can be seen, then, that the total resistance of the circuit must be 1,000,000 ohms. If *S* is about 25,000 ohms, a change of 10% in him would cause a current variation of only 0.2%. The potential would fall along the current in proportion to the resistance. If 50 v. are used through a resistance of

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*From the Psychological Laboratory of Columbia University.

[1] This device was designed with the help of Professor S. L. Quimby, of the Department of Physics, Columbia University.

1,000,000 ohms, the drop across an S of 25,000 ohms would be 1.2 v. A decrease in resistance would decrease voltage drop in proportion. This voltage is led to the grid of an amplifier tube.

A variable opposing E.M.F. C_2 is inserted to keep the amplifier tube operating on the straight part of its characteristic (as indicated by the recording instrument, M_r). The plate current of this tube, determined by the voltage on the grid, is led through the measuring instrument (M_r), with or without further amplification.

A FURTHER MODIFICATION OF THE SANFORD CHRONOSCOPE

By FORREST L. DIMMICK, Hobart College

Everyone who has occasion to use the Sanford pendulum chronoscope is aware of certain disadvantages inherent in the instrument, not the least of which is the impermanent and variable nature of the mode of suspension of the pendulums. Students in the elementary laboratory are unable to adjust the threads accurately in a reasonable time and even after adjustment is attained it lasts for only a short time. It is customary to have the student check the accuracy of the pendulums at the beginning of every laboratory period, but in the writer's experience, the relative accuracy of the original adjustment does not last much beyond fifty reactions. To overcome this particular difficulty we have worked out the following modification in the Hobart Laboratory (see Fig. 1).

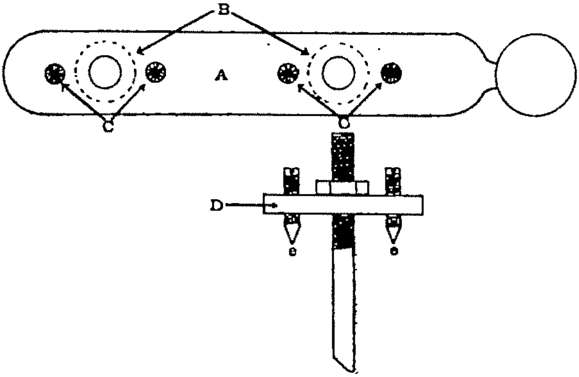

FIG. 1. SHOWING THE MODIFICATIONS OF THE SANFORD CHRONOSCOPE

We removed the brass arm which carries the pendulums and substituted a flat piece (A) of the same length and 1/2 in. by 3/16 in. Through this new arm we drilled two tapered holes (B) 1/4 in. to 1/2 in. in line with the two release buttons. At a distance of 5/16 in. on both sides of these holes we made shallow conical pits (C). At the top of each pendulum bob we drilled and tapped a 1/8 in. hole and into it fitted a 1/8 in. brass rod threaded at both ends. The upper end of the rod we passed through one of the holes in the supporting arm and into a cross arm (D) which carries two adjustable points (e). These points fit exactly into the depressions on either side of the hole. When the pendulum is adjusted to its proper length it swings freely and without friction on its point bearings. With this type of suspension, a pendulum will swing much longer than with thread suspension.

The introduction of the brass rod in place of the threads raised the centers of gravity of the pendulums. This necessitates lengthening them a little. We have found it sufficient to introduce a 1/16 in. washer at the base of the vertical column to assure the clearance of the longer pendulum. Also, because of the rigidity of the pendulum it is necessary to use a double link between the bob and the release clamp.

The writer has found that this modification, which is a return to the principle originally used by Sanford,[*] retains its accuracy of adjustment over long periods. The first instrument which was altered in this laboratory to try out the arrangement was in perfect adjustment after two months. Following this pattern, a local mechanic rebuilt three others at a total cost of $9.75 and constructed six new ones for $35.00.

A Simplified Electromagnetic Aesthesiometer

By Wilbur S. Hulin, Princeton University

Electromagnets are often used in aesthesiometric work for the determination of small and precise temporal intervals.[1] The models designed by Benussi[2] and Wirth[3] are representative of those used for two-point stimulation. There are two difficulties, however, in the use of these models. First, it is difficult to eliminate all vibration, impact and noise if the stimulus point is thrust on the skin by the make of the current[4] or by the release of a spring.[5] Secondly, it is often impossible to compare or duplicate the intensities of the stimulations because they are measured by the strength of the current.[6]

The aesthesiometer shown in Fig. 1 was designed by the writer to eliminate sound and jar.[7] The instrument consists of a solenoid within which an iron plunger is held suspended as long as the circuit is completed through the coil.

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[*] E. C. Sanford, A simple and inexpensive chronoscope, this Journal, 3, 1890, 174-181.

[1] The 'tactual stimulus' (no. 23283) made by C. H. Stoelting is an example.

[2] V. Benussi, Kinematohaptische Erscheinungen, *Arch. f. d. ges. Psychol.*, 29, 1913, 385; K. M. Dallenbach, A kinohapt, this Journal, 34, 1923, 90-92.

[3] Cf. O. Klemm, Über die Wirksamkeit kleinster Zeitunterschiede im Gebiete des Tastsinns, *Arch. f. d. ges. Psychol.*, 50, 1925, 207.

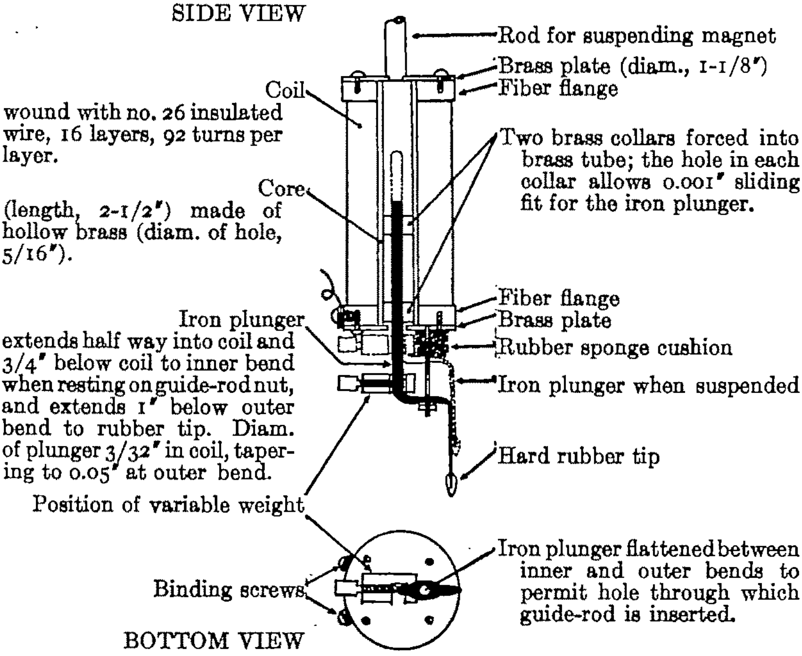
[4] Benussi and Stoelting; also, H. E. Burtt's apparatus, *J. Exper. Psychol.*, 2, 1917, 372 f.

[5] As in Wirth's apparatus; cf. Klemm, *op. cit.*, 207.

[6] A. K. Whitchurch, (this Journal, 32, 1921, 478) improved this situation by using Von Frey hairs. The writer (*J. Exper. Psychol.*, 10, 1927, 298) allowed the stimulus point to fall by gravity so that the intensity could be measured in grams, but the fall, determined by the magnetic pull on a suspension rod, involved a disturbing vibration.

[7] This instrument has been constructed by Mr. Wm. Habada at Princeton University. The above references illustrate the use and control of this instrument.

When the circuit is broken the iron plunger (*i.e.* the stimulus-point) descends upon the skin by gravity. The intensity of the stimulation is determined by detachable brass slots (grooved so as to be fastened with a set screw to the iron plunger above its inner bend). Slots weighing up to 10 grm. can be employed with a current of 10 v. if the dimensions shown in Fig. 1 are used. With weights of 5 grm. or less a 7 v. current is sufficient, and this current can be maintained for an experimental hour without unduly heating the coil. The stimulus-point is set out to the edge of the solenoid so that the points of two such aesthesiometers may be brought together for a zero spatial separation. The brass guide-rod prevents the iron plunger from pivoting around, and the nut at the lower end of the guide-rod prevents the plunger from falling out altogether when the current is broken.[8] While the plunger and appended weight are permitted a maximal excursion of more than half an inch, the plunger properly should not fall more than half a millimeter to the skin so that the skin will merely be pressed and not struck with a jarring impact. The small excursion is determined by the amount of rubber sponge which is transfixed on the upper portion of the guide-rod.


SIDE VIEW
Rod for suspending magnet
Brass plate (diam., 1-1/8")
Fiber flange
Coil wound with no. 26 insulated wire, 16 layers, 92 turns per layer.
Two brass collars forced into brass tube; the hole in each collar allows 0.001" sliding fit for the iron plunger.
Core (length, 2-1/2") made of hollow brass (diam. of hole, 5/16").
Fiber flange
Brass plate
Rubber sponge cushion
Iron plunger extends half way into coil and 3/4" below coil to inner bend when resting on guide-rod nut, and extends 1" below outer bend to rubber tip. Diam. of plunger 3/32" in coil, tapering to 0.05" at outer bend.
Iron plunger when suspended
Hard rubber tip
Position of variable weight
Iron plunger flattened between inner and outer bends to permit hole through which guide-rod is inserted.
Binding screws
BOTTOM VIEW

(one half actual size)

FIG. 1. A SIMPLIFIED ELECTROMAGNETIC AESTHESIOMETER

apart from human experience, are correctly regarded as susceptible of examination apart from human valuation and preference. But when events which, by definition, are considered as depending upon an experiencing individual become the subject-matter of investigation, it must then naturally follow that the meanings and values with which individual experience is shot through attach themselves as integral properties of the events under observation. A value, if it is in human experience, is a fact. So is a meaning. As such, they both are just exactly as observable as sensory materials. Were it otherwise, how should we ever know that there were such things as values and meanings?

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[8] The single rod (diam., 1/16 in.) inserted through a hole in the iron plunger obtains far greater silence than a U-shaped rod embracing the plunger.

Existential psychology, in line with the dictates of critical science, declines to offer an explanation of its facts. This refusal to explain is often qualified by reminding the reader that what is meant is explanation in the popular sense. Just what the difference is between scientific and popular explanation remains at times a bit obscure. Popular explanation is said to seek for causes, whereas scientific explanation seeks for correlates. Well, to drive a significant and clean-cut distinction between causes and correlates, if possible at all, is a task for the logician rather than for the scientist. In trying to avoid the dangers which lurk in the phrase 'cause and effect' existential psychology has come dangerously near to pouring out the baby with the bath water. To strip causal interpretations of their gratuitous assumptions about mystical forces is not by any means to give up the attempt to find the explanation of events. Science is as much interested today in the *why* of events as it was two hundred years ago. The principal change has been in the direction of refinement of technique and in a clearer understanding of what is meant by the so-called causal relationship. The assertion that one event is the cause of another turns out to be merely a way of saying that the two events, under the conditions of the experimental situation, are uniformly related in observation. The attempt of existential psychology to limit its task to pure description of bare sensory process strikes me as an almost futile piece of business, as far as the advancement of psychology is concerned; for it amounts to a flat proclamation that the conditions which are responsible for the occurrence of a given sensory datum (inasmuch as it is obvious that these conditions do not reside in the sensory stuff itself) are to be completely ignored. Existentialism admits the presence of a logical relationship (whatever that is) between sensory material and neural processes, but the implication is near at hand that the study of these relationships is more properly the task of empirical psychology.

As a corollary of the disregard for explanation, existential psychology refuses to deal with prediction, for it is not the purpose of critical science to predict events. But even if critical science confines itself to bare correlations it is already on the road to prediction. A correlation, to be of any scientific significance, must be stated in numerical terms. If the correlation is low then predictability is correspondingly low; if the correlation is 100% then predictability is absolute. The one naturally follows from the other.

My chief quarrel with existential psychology relates to the distorted picture which it offers of direct experience. I am heartily in agreement with it in believing that the subject-matter of psychology may very properly be defined as direct experience regarded as dependent upon an experiencing individual. But to maintain that direct experience is identical with meaningless sensory process strikes me as flagrantly contradicted by everyday observation. So seldom do I bump into bare sensory stuff in my own experience (even under the artificial abstractions of laboratory observation) that I consider it very decidedly the exception rather than the rule. I find in my direct experience such things as houses, books, trees, fires, danger, prejudices, likes and dislikes, all sorts of values and meanings, and I note too, in direct experience, the responses of my own body and the movements and activities of other organisms. All of these events, and many many others, spread themselves out in direct experience. It was a magnificent hypothesis that all of this rich and buzzing confusion could be analyzed down into sensory elements of consciousness. But the history of psychology has seemed to show pretty conclusively that the hypothesis was inadequate to the task. It will be salutary, I think, for psychology to cast off this hypothesis once and for all, if for no other reason than that adherence to it is likely to check a free and adventurous spirit of inquiry in a science which is too new and insecure to allow itself to be hampered by rigid rules as to what a science should and should not do.

The objects of direct experience fall roughly into three groups; (1) stimulus-objects, or those events to which we ascribe an existence apart from our observation of them; (2) mental objects (the name is immaterial), or those events which seem peculiarly to be our own private experience; and (3) biological objects, or the movements of our own bodies or the bodies of other individuals. All of these events or objects, as given in direct experience, are obviously related in some way to the operations of a nervous system. Hence it would seem reasonable to argue that the science of mind must concern itself with the interrelations among the above objects and with the dependence of these objects and their relations upon underlying neural processes. Any narrower definition runs the risk of excluding the examination of those very conditions which may play a part in determining the properties of the objects which fall within the definition.

Harvard University CARROLL C. PRATT

La causalité physique chez l'enfant. By JEAN PIAGET. Seventeen Collaborators. Paris, Félix Alcan, 1927. Pp. 347.

This is a more detailed analysis of the child's conception of the physical processes than the author's earlier volume, *La représentation du monde chez l'enfant*, published the previous year. The author and his students and former students present the reasoning of children as brought out by certain standard questions rather informally encountered in individual conversation with *E*. Three general methods are used: (a) The child is asked whether certain bodies or objects have force and why. (b) The child's attention is directed to movements as of the clouds, of the parts of a machine, etc., and he is asked to comment on and explain them. (c) Certain simple physical experiments are carried out before the child and he is asked to explain what happens. This third method is less verbal than the others. It is said to have arisen from numerous experiences rather than from preconceived notions. We may make clear the method of the present investigation by some illustrations from the experiments relative to the nature of air. After some informal preliminaries, the following simple experiments are carried out, and the child's reactions and explanations are skillfully drawn out: (1) In a small lid suspended by strings a *sou* is placed and the lid is rotated in a vertical plane. The child is asked why the *sou* does etc., so that a philosophy of the science may be constructed. While he does not at present attempt to construct such a philosophy of psychology, he endeavors to sketch briefly the genesis of different fields and varieties of psychology and the contributions of different leading personalities to its development, as well as to analyze critically certain of its conceptions and methods. The great variety of the conscious experiences of various animals and of children and adult humans of the two sexes, and of different ages and races are constantly stressed. His uncritical method of establishing sensations in different higher animals is somewhat curious. He argues, for instance, that the lion must perceive its hunger, else why does it pursue its prey. His neglect of recent knowledge on inner drives as effected by metabolic changes greatly complicates his difficulties of explanation. While he does not neglect to consider physiological bases he fails to see the simplification that its possible if only he would conceive these activities mechanistically in some fashion. Going into the conscious aspects of the individual, he points out that no two individuals are alike; that they vary from certain general norms to all sorts of abnormality. To understand any one person fully would take a lifetime. Children are not simply diminutive adults, we are reminded, but have their own unique experiences at each stage of their development. The sensory key-board (clavier sensoriel), as it were, of the normal individual—running up from the musculo-tactile zones through the auditory, gustatory, olfactory, thermal, and visual receptors—is briefly considered, together with modes of stimulation, degree and variety of development in the different modalities with respect to man. The possibilities of further development and refinement of some sensory fields are considered, and the author shows leanings toward the Lamarckian view of heredity, citing the now repudiated work of Pavlow on supposed hereditary transmission of conditioned reflexes. The sporadic cases of continued sensory development through a few generations, attributed to hereditary effects of training, can readily be accounted for otherwise. Still inclining toward the old doctrine that all knowledge comes through the senses, and holding to the sensory-associationistic view of intelligence, which conceives intelligence as an accumulation of 'ideas,' the author examines critically some of the elementary forms of experience and the elementary processes in the acquirement of ideas.

Imagination is divided into actual and residual, the former being that which converts the sensory effects of external stimuli into images, while residual imagination refers to the perseverance of images previously aroused. The number of images which one may have at any time depends upon (1) the *finesse* of the sensory organs, (2) the variety of stimulation, and (3) the degree of attention of the observer. The nature, composition, and exactitude of images vary infinitely. There are three kinds of imagination—practical, esthetic, and scientific—we are informed. There are several sorts of attention both active and passive, peripheral or central; and attention with effort (active attention) seems to be for van Biervliet, a real, spontaneous force to be reckoned with, as it has always been for most of the sensory associationists, who approached our obviously dynamic mental activities from an introspective rather than from a objective, genetic point of view. There is, of course, no question about the reality of this 'spontaneity' in our experiences: the question concerns its interpretation, a matter that is neglected in this little book. Attention plays an important rôle for the author both in imagination and in intelligence, but conceptions of a general memory are rejected. We have 'memories,' not 'a memory.' The degree of intelligence depends upon the *finesse* of the sensory equipment, the "power of attention," and the effectiveness of the memories. This useless complication, due to 'explaining' certain assumed arbitrary powers by others equally inaccessible to scientific investigation, greatly mars the author's psychology. Three classes of intelligence, like those of imagination, are enunciated and described—practical, esthetic, and scientific—but no evidence is given for accepting these particular ententies or types to the exclusion of others that have been or may be imagined.

Introspection gets a very good, critical consideration in one chapter. Three main classes are indicated: (1) everyday incidental self-observation, (2) the trained self-observation of psychologists, and (3) the self-observation by trained psychologists under carefully controlled conditions in which different relevant factors may be varied according to well planned experimental procedure. But introspection at best is limited by the fact that we note in the main only what interests us and we become set for what our own theories and views hold to be real. Moreover, many important processes contributing to our behavior lie beyond the field of our own direct observation. The author cites the study of unconscious reflexes as having yielded by objective methods a whole group of important facts about ourselves which could not have been obtained introspectively. The introspective method, however, is not wholly discredited. It is "indispensable for the study of the conscious life. The experimenters are the last to deny this. For certain of their researches it is absolutely capital." A chapter is also devoted to the method of inquiry, including the questionary used by Galton and many others. This method is misused, we are told, whenever the subjects are not qualified to make serious critical examination of the facts investigated; and in any case certain conditions and precautions are carefully to be observed in the use of the method, whether orally or by means of written procedures and forms.

The methods of tests, scientific observation, and experimentation receive attention each in a special chapter. The test method, says the author, appears to avoid the principal defects of the inquiry methods (*des enquêtes*). Tests are objective and simple; they are quickly and easily employed, and furnish concise and decisive results. But they are often used badly by persons who have neither adequate training or the sufficient preparation to understand their theoretical basis. Group testing, especially, is criticized on account of inadequate instructions beforehand, rough, wholesale procedures, insufficient opportunity to take note of individual work as it goes on, and the losing of individual characteristics in most results in the form of totals. Extensive individual tests, well worked out by preliminary experimentation, are favored. The work of Ribot on diseases of memory, of Janet on automatisms, and of Dumas on the mechanisms of emotions is cited as illustrative of useful scientific work which may be judiciously carried out by the observational method. On such work one might well impose certain conditions, as the author does on introspection, to insure objectivity. Under the head of the experimental method, which is recognized as of the greatest importance, the author cites his *Psychologie quantitative* for a fuller treatment, and he gives only a brief sketch of the so-called psychophysical methods and mentions certain considerations and precautions meriting the serious attention of experimenters. The numerous science, philosophy of history, philosophy of religion, and aesthetics, Boutroux discusses these movements, finding as the net result for the period in question "not an evolution but the disappearance of philosophy."

In the section on psychology, the author considers very briefly the foundations laid by Taine and Ribot which resulted in the development of pure psychology (Marillier, Paulhan, Godfernaux, Ruyssen) introspective psychology (Egger, Compayré, Marion, Derepas, Dugas, and Malapert), the study of psychic automatisms and hypnotism (Richet, Janet, Grasset), social psychology (Tarde, Le Bon), and religious psychology (Récéjac, Delacroix).

Vanderbilt University HERBERT SANBORN

The Modern Cat, Her Mind and Manners: An Introduction to Comparative Psychology. By GEORGINA STRICKLAND GATES. New York, Macmillan Co., 1928. Pp. ix, 196.

This is a collection, we are told, of true cat stories, "intended for two classes of readers: those who love cats and would like to know more about the explanation of their actions, and those who may wish to obtain, through a description of methods employed with one animal, a first glimpse of the ways of comparative psychology." The book suffers from this double purpose, with sentiment predominating. "Modern" in the title has no significance other than comparison with "modern woman" and "modern marriage." One looks in vain for a biological account of the evolution of the cat. The cat's leisure, its freedom to wander about, and its lack of economic value are contrasted with conditions found in the lives of certain other domesticated animals, and the cat is said to be even better fitted for wild life than the dog. The cat's status with man has varied from that of a sacred animal or even a deity to that of the proverbial "black cat" and associations with witchcraft. Treatments of the cat by both friendly and unfriendly "critics" are illustrated. The feminine point of view is everywhere evident, as when the author alleges that "there is scarcely a person who does not stop to pet the cat in the grocery store." It would have been interesting for our author to inquire into the question as to why she, as a representative of her sex, should display this cat-interest, so much more rare in men.

Certain well known laboratory tests and experiments with the cat are recounted to show how this modern creature has even "stepped into the scientific laboratory." A number of tales about cats are given as examples of material which the comparative psychologist cannot use. Several of them are employed effectively to illustrate the limitations of untrained observers. The methods and results of laboratory studies of the cat are outlined, and the learning of this animal is compared with that of the dog and of some other animals. The cat is listed as probably equal to the dog, brighter than the horse, the chick, and the rat, but inferior to the monkey. Some of these conclusions are, of course, only tentative. The author leaves open the question as to whether the cat has "ideas," but suggests that "ideas," "thoughts," etc., may be possessed by it but not frequently used. She is not free from the assumption that ideas are things which may guide action. Good accounts and discussions are given of tests and experiments on the homing of the cat, and of its repertoire of unlearned activities. These are cleverly presented amid many amusing superstitions about certain actions of the cat under given circumstances as "signs" of future events. Cat lovers will doubtless be somewhat disappointed in the results of experiments on the cat's vision, the revelations of the scientist as to the nature of its emotional life, and its generally low level of mentality. Sixty-three references are given. The book has some good illustrations of the cat at work in nature and is clearly written. It should serve the valuable function of indicating to the general reader how scientific methods are gradually penetrating the realm of behavior, and how they make behavior, even in animals, intelligible and inviting as a field of study and control. J. P.

Coming of Age in Samoa. By MARGARET MEAD. Foreword by Franz Boas. New York, William Morrow & Co., 1928. Pp. xv, 297. 8 Pl.

Miss Mead assumed, on *a priori* grounds, that the adolescent period is inevitably one of mental stress and conflict for children who are living in America. This suggested to her the question, "Are the disturbances which vex our adolescents due to the nature of adolescence itself or to the civilization?" To answer it she studied the natives of American Samoa, particularly the adolescent girls, under native conditions for a period of nine months. She reached the conclusion that adolescence is not "a period of mental and emotional distress for a growing girl as inevitable as teething is a period of misery for the small baby" (p. 196).

This she believes to be true largely as a result of the "simplicity" and "naturalness" of life in Samoa. There, the adolescent girl has no difficult choices to make nor has she any inhibitions concerning sex, birth, or death, which are the factors held most responsible for the difficulties experienced by American children.

The argument, as presented, is weakened somewhat by the number of cases of mental disorder which Miss Mead, who admits being only an amateur in psychiatric diagnosis, discovered and has recorded in the appendix. Although her presentation does not lend itself readily to statistical interpretation it seems probable from the evidence the author herself presents that at least one percent of the total population is suffering from some form of mental disorder (not counting feeblemindedness or even cases of marked sexual abnormalities which are common). Rather more than would be expected under really 'simple' conditions!

The book contains a description of the family life and the social institutions of the Samoan people, which is interesting and well done. For this reason a sociologist might consider it more of a contribution than would a psychologist; at least it seems to contain more of interest and of value for him.

Stanford University ROBERT G. BERNREUTER

About Ourselves: Psychology for Normal People. By H. A. OVERSTREET. New York, W. W. Norton & Co., 1927. Pp. 300.

The author indicates the readers for whom the volume is written, as follows: "Is the reader hopelessly perfect? If so, let him close this book. It is not for him. Is he so mentally ill that he requires the careful attention of a psychiatrist? If so, again, let him close the book." "This book is for the rest of us, the in-betweens—the so-called 'normal.' It is written out of the conviction that we normal persons need to know a good deal more about ourselves than we ordinarily do."

The book is a non-technical, interestingly written volume that will appeal to the general reader. The style is rather conversational and intimate. Through this combination the author comes to the reader as a friendly counselor for the smaller mental ills that sometimes reduce the efficiency and happiness of our lives.

In Part I is set forth many typical instances of these minor maladjustment. Biases, prejudices, fears, exaggerated self-importance, shunning responsibilities, abnormal sex tendencies, and many other similar traits are identified for the reader and their developments as habit patterns traced. These ills represent gradual habit accretions, ignorantly or inadvertently taken on by the individual. Since they give evidences of psychological weakness in the person's make-up, the problem is, how to build a strong constitution.

Part II sets forth some of the worthwhile yet frequently unused means by which strength and balance may be attained. Music, the objective arts, poetry, and conversation are treated in some detail both as to the effects produced and the manner in which these effects are brought about. The general point of view is that the ills are identified with introvert behavior tendencies, and that the remedy is the development of 'outward' reacting selves.

North Carolina College for Women J. A. HIGHSMITH

Mitteilungen der Kriminalbiologischen Gesellschaft. Graz, U. Mosers, 1928. Pp. 84.

As the title indicates, this is the report of proceedings of the first meeting (June 1927) of a group interested in the study of criminology from the inclusive standpoint of biology. In addition to a list of officers and members, the resolutions adopted, and a statement of purpose by the president, Adolf Lenz, there are six formal papers. All of these emphasize the necessity of making an exhaustive study of the problem by methods which shall include every scientific discipline which may contribute to its solution. Incidentally there are some interesting reports of institutional work in progress in several European centers. The problem of types appears prominently in their thinking, but with no failure to emphasize the large place of environmental influences in development.

University of Oregon EDMUND S. CONKLIN

The Psychology of Religion. By CHARLES CONANT JOSEY. New York, Macmillan Co., 1927. Pp. xi, 362.

This volume is a valuable elementary text-book in the important but extremely difficult field of the psychology of religion. The book is divided into three parts, of which the first is devoted to an introduction and definition, the second to the development of religion in society, and the third to a consideration of the growth of religion in the individual.

After a well conceived definition of the psychological approach to the study of religion, the author shows in detail the psychological facts that he considers important in dealing with religion as he defines it. A scheme accounting for the general psychological changes that may be observed in religion, when comparatively studied, is propounded. The changes in regard to the concepts of God and of immortality are analysed.

The gradual development of religion in the individual is considered, with special reference to adolescence, conversion, cult practices, preaching, asceticism, prayer, and mysticism.

It is very easy to treat the psychology of religion in such a manner that it at once repels the devout churchman. The writer finds in the psychology of religion, as it is often presented, a sharp antagonism to the beliefs that he holds most significant. This volume successfully avoids creating this hostile attitude. The result is, however, that certain portions of the book seem to the psychologist rather the social philosophy of religion than the psychology of religion.

Brown University LEONARD CARMICHAEL

Das autonome Nervensystem. By ERICH SCHILF. Leipzig, G. Thieme, 1926. Pp. viii, 207.

Starting from the foundation laid by Langley the author describes the autonomic nervous system as it presents itself in the light of experiments and observations in the fields of physiology, histology, pharmacology, surgery, and embryology. The psychologist will take note of the fact that current generalizations concerning autonomic innervation, the antagonism between the sympathetic and parasympathetic system, etc., have no, or a very narrow, experimental basis. Throughout the book the specific problems involved are clearly stated and experimental methods for their solution are suggested. The treatment of Schilf brings home to us that the assumptions concerning autonomic innervation as frequently found in current psychological literature do not even merit the name of an hypothesis. As long as the study of the autonomic nervous system yields as the chief result a large number of disconnected empirical findings, it is difficult to see how the experimental formulation of psychological problems can derive any benefit from the results of a premature synthesis of such findings. Schilf's presentation makes clear that there is hardly any basis for broad generalizations of psychological importance.

Institute for Juvenile Research HEINRICH KLÜVER
Chicago, Illinois

Keeping Mentally Fit. By JOSEPH JASTROW. New York, Greenburg, 1928. Pp. ix, 297.

This book is a collection of articles which have appeared daily in a group of newspapers. Quite consciously the style is journalistic, the sections brief and an effort made to hold the interest for a short time and during that time give the scientific solution of some specific problem in an understandable way. The author even resorts to answering letters which appeared—or at least might have appeared—in the 'personal' columns of the papers.

As the title would indicate, this is an effort to give mental hygiene to the public. The author is concerned with sections on the control of the emotions, mental hygiene of the child, analysis of traits of personality and the like. His contribution to mental hygiene is not only to give positive advice in regard to these matters but also to break down a large number of popular misconceptions which have grown up about psychology and about mental processes and reactions. In the reviewer's opinion, Jastrow has succeeded admirably in presenting these matters at the low level of the average reader of the newspapers.

University of Pennsylvania SAMUEL W. FERNBERGER

NOTES AND DISCUSSIONS

The Lure of Absolute Objectivity

In the realm of physics, it is axiomatic that action is followed invariably by reaction. Among social phenomena, a similar law seems to operate. In the fields of education and the other social sciences, there are numerous evidences today of potent reaction. A period in which complacency characterized educators was followed by an alleged scientific attitude. The scientific attitude itself is a product of another reaction, namely, the objective point of view urged by certain psychologists. For centuries, man was accustomed to speculate and to theorize with reference to human nature. Gradually man's thinking became more critical and within recent years the *ideal* of rigorous objectivity has been promulgated. Indeed, it is not an exaggeration to say that the ideal of *absolute objectivity* has been advanced and that this ideal is now seriously espoused by certain students of psychology.

For example, Watson asserts that the *behavior* of man is the subject matter of psychology; he insists that behavior should be studied and recorded objectively. Because so many inferences based upon subjective evaluation have proved to be fallacious, Watson refuses to employ *inferential thinking*! Since he refuses to *infer* the existence of integers which are not amenable to objective verification, he refuses also to recognize or employ such concepts as consciousness, perception, imagery, will, etc. He avers (truly enough) that the concept of consciousness is an inference; he assumes that such inference is 'unscientific.' Watson is not alone in urging that the human being can and should be judged by his behavior. Many insist that the methods employed in psychology and education should be those which characterize the natural sciences and that these methods are clearly objective in nature.

The attitude of the behaviorist implies that certain natural sciences are largely if not wholly free from the use of subjective methods; that the natural sciences therefore have the prerogative to designate themselves 'sciences.'

> "So far in his objective study of man no behaviorist has observed anything that he can call consciousness, sensation, perception, imagery or will. Not finding these so-called mental processes in his observations, he has reached the conclusion that all such terms can be dropped out of the description of man's activity".[1]

The premise of the behaviorist, stated above, merits study and consideration. No science (not even chemistry or physics) employs invariably the objective method. The concept of absolute objectivity may be a worthy ideal, but the conviction that psychology can (or should) make *no* use of inferential thinking is fantastic, for even the natural sciences make extended use of inferential thinking. Complete objectivity does not characterize the work of present-day scientists in the fields of chemistry, physics, or biology. The workers in these natural sciences employ inferential thinking in many instances;

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[1] J. B. Watson, Behaviorism, *Encyclopedia Britannica*, 13th. ed., 345.

often the evidence used to substantiate their inferences is vague and inadequate. In numerous instances, major premises are admittedly assumed.

In this connection it will be of interest to turn to those who are best qualified to speak in behalf of the natural sciences. Several representative scientists have collaborated recently upon a volume intended to orient the beginning student in his scientific thinking.[3] It is of interest that the representatives of the natural sciences assert that they *employ their imaginations* in coördinating the facts obtained by experimentation. Even the chemists (supposedly the most objective of scientists) speculate regarding their data. Stieglitz makes the following statements:

> "Let us also understand that chemistry is an experimental science, evolving its thought and testing its conclusions with the aid of experimental work. You will find too that in coördinating the facts and teachings of experiment, chemistry is *compelled* to draw on our powers of *imagination*, controlled by the critical questioning of our intelligence, for chemistry has found the most satisfying interpretations of the visible, gross observations of our senses in *conceptions* involving rapidly moving material particles which are far too minute to be individually seen, weighed or counted. It is because the whole thought of chemistry is evolved from the application of intelligence and *imagination* to experimental results, that we shall develop in these lectures our own study of the nature of chemical processes with the aid of experiments which should bring home to you the basis and the nature of chemical reasoning" (pp. 119 ff.).

In the preceding quotation, the writers of this paper have italicized words which reveal the subjective nature of the chemist's work. The chemist draws upon his *imagination*, because, according to his *belief*, he is dealing with "material particles which are far too minute to be individually seen, weighed, or counted." The chemist's imagination is controlled of course by critical questioning. Thus the chemist deals often with data which are *not directly observable*; he is able to coördinate his findings only when he draws upon his imagination. If it be assumed that the psychologist must use the same methods as do the chemists, it is paradoxical to assert that inferential thinking is untenable in studying human behavior. The postulates of Watson are obviously not those of certain chemists. The chemist will be found to be less objective; he relies upon *assumption*, *supposition*, and *conjecture*. In the following quotations the writers have again italicized words which indicate that the chemist deals with integers which he does not and cannot observe directly.

> "It is *assumed* that in the hydrogen atom the massive but minute proton is in the center of the atom and attracts the electron which probably revolves around it. . . . Each atom of the other elements *is considered to contain* a nucleus built up of protons and electrons, and this nucleus represents the chief mass of the atom. The exact character of the forces holding protons and electrons in nuclei *is not known*" (p. 129).
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> "An electron doublet *is supposed* to move closer sometimes to the one, sometimes to the other, atom. For reasons which we have not the space to develop here, it will be safe *to assume* that the doublet is without question movable, but that in the molecules of very many compounds it remains definitely much closer to the kernel of the one atom than to that of the other atom, as a result of the net sum of all electrical forces" (p. 138).

It may be that the preceding statements are not made by a behavioristic chemist. One is led *to suppose* that a behavioristic chemist would be less rash.

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[3] H. H. Newman and others, *The Nature of the World and of Man*, 1926.

than the natural sciences is doomed to undergo marked modification. If behaviorism is to receive general acceptance it is clear that it must relinquish many of the excesses which characterize it.

Behaviorism arose of course from the desire for objective, simple, and demonstrable data and conclusions. The behaviorist has had a wholesome influence in the fields of psychology and education. Certain eddies from this new current already have waxed and waned. They have arisen because they offered certain attractive features. They have disappeared because they rested upon assumptions which were not tenable. A few years ago, Rugg protested against what he termed 'unscientific' curriculum construction. He insisted that curriculum-making should and could be placed upon a strictly scientific basis. Rugg's 'scientific procedure' for assembling material for a curriculum in the social sciences included the following steps:[3] (1) a study of textbooks now in use; (2) a study of what 'understanding thinkers' would include in the curriculum; (3) a study of allusions found in various publications; (4) a study of what is crucial matter in present curricula.

Schafer showed that the above steps are lacking utterly in scientific precision.[4] For example, one can not determine crucial subject matter (Step 4) without employing subjective judgment. Nor can one identify 'understanding thinkers' (Step 2) except by opinion. Rugg ultimately gave up the term 'scientific' in connection with his work explaining that he found it misunderstood.[5]

Perhaps the most fantastic of all claims for absolute objectivity was that of Washburne and his collaborators when they attempted to construct 'scientifically' a factual course in the social studies. It is indeed incredible that Washburne should have thought that he could construct a curriculum without employing judgment and critical evaluation of objective data. According to Gambrill, this is precisely what Washburne attempted. Gambrill's remarks follow:[6]

> "The object of Superintendent Washburne and his Social Science Seminar is to construct a curriculum by a strictly scientific procedure without any dependence upon opinion or judgment. . . . The problem was to select in a purely objective scientific manner the proper names and dates that would meet these requirements and from them to construct the desired curriculum."

The members of the Winnetka Seminar read through a large number of periodicals and recorded every allusion to a person, place, event, or date (excepting only advertisements). The allusions were tabulated and each was given an index number based upon frequency of allusion. An attempt was then made to construct a factual course of study in the social sciences which would enable the pupil to understand allusions to the persons, places, events, and dates which appeared frequently in periodical literature.

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[3] H. O. Rugg, Needed changes in the committee procedure of reconstructing the social studies, *Element. School Jour.*, 21, 1921, 697f.

[4] J. Schafer, The methods and aims of committee procedure, *Histor. Outlook*, 12, 1921, 247-249.

[5] Quoted by J. M. Gambrill, *Experimental Curriculum-Making in the Social Studies*, 1924, 13.

[6] *Op. cit.*, 24.

Gambrill has set forth a list of questions which shows the utter untenability of Washburne's procedure. The questions asked by Gambrill follow:[7]

> "What does it mean to understand an allusion? If it be assumed that the index figure is a measure of importance, does it follow that the amount of time or emphasis in the school should necessarily follow that figure? Should no allowance be made for what is learned outside of school? Mr. Washburne thought not, on the ground that the procedure could be *scientific* only if the index figure be followed absolutely, and the element of personal judgment entirely eliminated. 'When an adequate means of measuring this (out of school learning) has been determined, we'll be glad to allow for it in all subjects.' "

Washburne's ideal of a strictly scientific procedure in curriculum construction is so extreme that it represents thinking as uncritical as that against which it protested. The initial assumptions in the Washburne plan are of course of doubtful validity. It is exceedingly unlikely that the importance of persons, places, events, and dates is proportionate to the newspaper space which they receive.

The preceding examples of attempts to be 'scientific' show that the educator is greatly annoyed by the prospect of not being considered a scientist. The idea seems to be prevalent that education does not deserve a place in the sun unless it can demonstrate that it employs strictly scientific methods. Scientific procedure is assumed to be mechanical, objective, and absolutely conclusive; there appears to be a wide-spread opinion that subjective judgments of every kind are taboo and that the true scientist never goes beyond his data.

There is much similarity between the practice of the 'scientific educator' and the behaviorist. The scientific educator announces principles of procedure which he is unable to follow consistently. The behaviorist condemns a psychological method which he employs in order to arrive at his condemnation. Although the behaviorist uses introspection at the outset (thus violating his principles), he refuses *theoretically* thereafter to employ this method. This refusal is very convenient, for it enables him to avoid facing his own inconsistency.

Although the behaviorist considers introspective reports to be invalid when used by his opponents, he refers to them as "positive evidence" when such reports are used to substantiate his postulates. Under the caption, *"Some positive evidence for the behaviorist's view"* Watson wrote the following in 1925:[8]

> "I have collected considerable evidence that those deaf and dumb individuals who when talking use manual movements instead of words, use the same manual responses they employ in talking, in their own thinking. . . . Dr. Samuel Gridley Howe, Superintendent of the Perkins Institute and Massachusetts Asylum for the Blind, taught the deaf, dumb, and blind Laura Bridgman a hand and finger language. He states (in one of the annual reports of the institute) *that even in her dreams Laura talked to herself using the finger language with great rapidity.*"

Watson describes the data given above as *positive evidence* for the behaviorist's postulates. One wonders by what right the behaviorist employs the term *positive evidence.* Watson may have arrived at his conclusion by means of an introspective account given by Miss Bridgman; on the other hand, Miss Bridgman may have been observed during her sleep and the observer may have *inferred* that the finger movements paralleled the dreams. In either case, the data are certainly not objective ones which are amenable to verification.

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[7] *Ibid.*, 27.

[8] J. B. Watson, *Behaviorism*, 1925, 193.

of the image as it would in the case of an after-image. Another point of interest in this connection was the fact that the results were unchanged even though the spectacles were removed five hours before the trial. Once the glasses were not worn for twenty-four hours previous to the observation, but the image appeared as before.

During observations made at later times it was noticed that if the background (wall of the darkened room) did not have any partially illumined area (*i.e.* a window), the entire image as diagrammed above could be seen at once, instead of just the portion that had the window as a background.

After submitting the above report, the observer made incidental observations on this image of spectacle rims for more than a year. On one occasion, while ill with influenza, he removed his glasses on a Tuesday noon, wore them again Wednesday morning for about twenty minutes, and then removed them again until the following Monday morning. On Friday and Saturday evenings the image was noted under the conditions described above. Sunday evening he forgot to watch for the phenomenon, but Monday morning before the glasses were put on, the room was darkened by drawing the shades and upon turning off the electric light the spectacles were visible for a few seconds. Apparently the appearance of the image was unaffected by the long absence of stimulation. The spectacles had not been worn for 120 hours.

A second report is given of the image seen July 1928 under slightly different conditions, although the light conditions were similar to those existing when the image was first noted.

On the evening of this observation there was no moon and the stars were veiled in a thin haze. I was hiking down a steep-walled, forested canyon and the surrounding spruces shaded much of the faint starlight so that even my hand was invisible when held in front of my face. Only the faintly lighted sky could be seen. The phenomenon was noted about 10 P. M., and since no other light had been seen since dusk the eyes were dark adapted. I happened to take off my glasses, and a few seconds later noticed an image of the rims when I looked at the sky. Due to the broad expanse of sky, the entire portion of rims and bows that was observed in the former experiment (*i.e.* about an inch of the bows and the upper outside half and lower outside quarter of the rims)—could be seen at once when looking above the horizon. Further experimenting was not done as I was quite fatigued from the day's hike. In this instance, to repeat, the image was observed after dark adaptation had taken place. Heretofore the image had been noted only immediately after turning out an electric light, fading out as the eyes became dark-adapted. The characteristics of the image were the same as previously described.

In addition to G's own report a few laboratory observations may be cited. Nov. 6, 1928, G reported at the laboratory at 4 P. M. He at once removed his glasses and the following tests were tried:

(1) G went into an inner room which had been darkened. A little light came in at the edges of the curtains. He got the usual image at the natural position of his spectacles. He alternately opened and closed the two eyes and moved his head from side to side so that the image appeared in peripheral vision. The image began fading at 50 sec. and had vanished by 80 sec.

(2) After a short interval in an outer daylighted room G returned to the dark room and saw the image a second time. It lasted 60 sec.

(3) G next returned to the outer room and for 3.5 sec. looked through a window at the bright sky. He then returned to the dark room and again got the image which persisted for 80 sec.

(4) He again spent 1 min. in the daylighted room and returned to the dark room. The image persisted for 65 sec.

(5) After a brief interval of about 45 sec. in the light room he went back to the dark room. The image was very dim and lasted only a brief interval.

(6) Electric light was turned on in the outer room and for 3 min. G stayed under the electric light, but did not stare at it. Then he went into the dark room. The image was seen clearly. The experimenter then tried the effect of introducing light by raising very slightly the curtain into the outer room. Light, except a very little, caused the image to vanish; with return of dark, when the curtain was again lowered, the image reappeared. It was quite possible to admit and shut out the light quickly and to get an alternate vanishing and reappearance of the image. The image died out in 112 sec. (Total time since G entered room). This last experiment is interesting in its outcome since a slight amount of objective illumination caused the image to vanish. This result is consistent with the usual loss of the image as the eye becomes dark-adapted and a dim illumination occurs. The image apparently can *always* be seen under the right conditions of illumination, but those conditions are narrowly determined. The frames, it will be recalled, are seen as a velvety black against a lighter black.

In making the above observations G turned his head from side to side, using only one eye and getting the image in peripheral vision. He reported that he also gets the image in direct vision with both eyes open and directed straight ahead, although less of the rims is seen. Accordingly a second series of laboratory observations was made April 4, 1929, in which both eyes were kept open and directed straight ahead. Otherwise conditions were similar to those of November 6.

(1) G, who had been working all afternoon in front of brightly lighted windows, studying botanical specimens mounted on white paper, after removing his glasses walked immediately into the dark room. He was instructed to look straight ahead so as to get the image in direct vision, and to report when dark adaptation made it possible for him to distinguish articles in the room; specifically, when he was able to distinguish in the dark the book-shelves from the wall at which he was gazing. The image was seen at once and persisted 7.5 min. Adaptation proceeded very slowly, much more slowly than for the experimenter who was also in the room. The eye was not completely adjusted to the dark until 11.5 min. By that time objects could be seen rather easily, even sheets of paper on the writing desk. The objects began to appear rather suddenly, but none of them until the image of the spectacle rims had totally disappeared. The image itself, during the early part of the experiment, was quite as clear-cut as a percept. G felt exactly as though his glasses were on. He also felt that dark adaptation had gone on a little more slowly than usual, perhaps because his eyes were so completely light-adjusted at the beginning of the experiment.

(2) After a short period of light adaptation in an outer room, G returned again to the dark room. This time dark adaptation took place within 2 min. The image of spectacle rims as seen in direct vision vanished in about 1 min. *but it could be seen slightly longer in peripheral vision* when the head was turned to the side.

all of them and it cannot do this in the tridimensional space-world. One's perceived world is only a small part of his own reality. There is not a correspondence among the various equations of the different perceptual realities. but when the equation of reality becomes perfect such a one-to-one correspondence does obviously exist. If $x^2 + y^2 + z^2 = 100$, it matters little if any of these terms, x, y, or z becomes zero. The equation is still true, the sum remaining 100. So with the disappearance to any one of us of the tree, the mountain, or any part of reality. All our 'signs' are but aspects of one universal spirit. Query: Is this 'universal spirit' itself a dream, or *the* reality?

In the absence of J. A. McGeoch (University of Arkansas), his paper on the influence of degree of interpolated learning upon retroactive inhibition was read for him. By methods essentially those employed in his former work [see, *e.g.* the April number of this JOURNAL, 252-262] he found on 42 *S*s with 10-syllable lists of nonsense material learned under complete presentation with time limits of 120 sec. for original lists, and of 60, 90, 120, 150, and 180 sec. for interpolated lists, that the degree of 'retroactive inhibition' increases 14.5% with increases of interpolated learning-time from 60 to 150 sec. and that it falls from that point 8.76% when this time limit is increased to 180 sec. Decreases of but slightly more than this occur if the interpolated learning-time is increased to 240 sec. Thus neither relatively small nor relatively large amounts of interpolated learning gave complete freedom from the 'retroactive inhibition.' The reviewer would certainly interpret these results in another way than by the assumption of retroactive inhibition, which somehow acts back on and undoes certain learning fixed in the past. The paper itself did not, it should be made clear, insist on the sort of effect implied by the term used. But why not discontinue a term that is so objectionable in its implications of a particular interpretation which may be weak?

C. N. Rexroad (Stephens College), in a paper on objectives of the first course in psychology, reported that many students have a deep and sensible interest in psychology and not merely a superficial curiosity relating to such subjects as hypnotism. They desire to know how different persons interstimulate one another to behave as they do. The student, he had found, is inclined to believe that the 'mind' holds the key to such behavior, so he wants to know about the mind. He soon learns, however, under proper guidance in his study of psychology that the structure of the biological organism is an important determinant of behavior and thus he comes gradually to a real interest in the nervous system, glands, muscles, etc., in short to a scientific approach to psychology. Rexroad sees no difference between a scientific and a practical course in psychology. The scientifically determined facts have all sorts of application to life for the intelligent and thoroughly trained student. W. A. Wilson (Ohio State University) and F. C. Dockery (Ohio Wesleyan University) led in a discussion of this general subject. Wilson contended that it is necessary to make clear to ourselves what we are trying to accomplish before we can evaluate our methods, decide which tests to use, etc., and he suggested that the development in the student of some ability to use scientific methods is more important than the mere learning of 'facts' as given in the usual textbook. Elementary texts, he said, gave no adequate picture at all of psychology as it exists today, that is, of the problems and methods on which psychologists are actually at work in the laboratories; and they do not afford training in scientific methods. Dockery pointed out that there are several points of view from which the question as to what should constitute the content of the first course can be approached. The instructor, representing the science, desires to have students know the things that he is interested in and accept his point of view. The students' point of view, he held, is to know something about themselves, and they are willing to sit down and have it poured in like so many doses of medicine. Then, there is the point of view of instructors in other related departments, who want their students to have some 'psychological facts' instilled into them as foundation work for courses in these departments. Dockery has tried to take note of all these points of view. On the whole he prefers giving "real psychology" to something about psychology. He made the interesting statement that "many students are so set in their own opinion that they never will adopt mine." Is not such a statement indicative of a dogmatic procedure in teaching which is likely to insist upon certain things as facts without developing their real bases and the methods of their derivation and evaluation? And is such a method not one reason why students seem to some instructors ready to sit and have alleged facts 'poured in'? With these markedly different points of view before the Society, the discussion wandered at random from this to that opinion. On one extreme was an insistence on the acquirement of facts, while on the other it was held that students ought to be interested scientifically in the subject and given training in the use of reliable methods of evaluating alleged facts and hypotheses about mental life and behavior. It seemed to be generally agreed that the instructor should not try to make psychologists of all his students, since only a few of them would finally specialize in the subject; and also that most of the present textbooks are inadequate. On this latter point the agreement was not so complete. A free-for-all discussion of this sort probably reveals to a keen observer the reasons why students often find psychology so uninviting a subject.

C. R. Griffith (University of Illinois), in the first paper of the afternoon session, described his kinaesthetic pursuit meter. *S* traces a line on a rotating paper belt, while the carriage which holds the pencil is acted upon at irregular intervals by two magnets pulling sidewise in opposite directions at variable strengths of 2 to 10 lbs. Since the problem for *S* is to keep to the line, it is necessary for him to oppose the force from either side, as soon as it appears, with a counter-balancing adjustment of the muscles. A time-recorder marks on the paper a scale of 0.02 sec. units so that the degree of accuracy of tracing of the line is shown in an objective curve accurately timed. It is obvious that this meter may have useful applications in the psychology of industry.

L. H. Lanier (Vanderbilt University), on the reliability of time and rhythm tests, reported that the older subjective methods of the measurement of time intervals probably deserve the relative obscurity which they now enjoy, but indicated that the more quantitative methods—such as having *S* judge pairs of intervals as to whether the second is shorter or longer than the first, or having him reproduce an interval equal to one given—though they have the ear-marks of objectivity, have not been carefully evaluated experimentally as to their reliability. This seems to be true even of the now much-used Seashore tests, time and rhythm, so far as their author is concerned, and Lanier's own results and those of Ruch and Stoddard show for these tests a rather low reliability. The point emphasized in the paper is that these various quantitative procedures still lack something in the psychological aspects of their administration and use which it is very desirable to have, if they are to be used with confidence in psychological investigations. The consistency of results with any one method is a fundamental concern to experimental psychology. Preliminary results of six different experimental approaches to this problem of reliability of time-interval measurement yielded data which were far from satisfactory. These tests and results we do not need to consider here in detail. The Seashore time-intervals test, for example, which in an earlier determination by Lanier yielded on two applications to 130 college students a coefficient of reliability of 0.45, in the series of tests here reported gave, by the same method, a coefficient of only 0.39 on 20 male college sophomore students in experimental psychology in New York University, despite a careful following of the author's instructions. The Seashore rhythm test on this same group gave a coefficient of 0.42, as compared with that of 0.50, which was earlier reported by Lanier on 130 college students. In both cases the scores in two separate applications of the test were correlated. The paper closed with an emphasis on the necessity of having very careful and exhaustive experimental work done toward discovering means and methods of raising the reliability of tests of this nature. In the discussion which followed it was brought out that the effect of different degrees of practice on reliability of such tests should not be neglected in the work suggested.

Following a paper by J. L. Leggett (Transylvania College), comparing the test records of students entering with their remaining through college to the point of graduation, and showing the usual greater probability of graduation by those who received a score above the third than below the first quartile, H. C. Sanborn (Vanderbilt University) discussed the important topic of democratic control of education. He traced the developments from the time when "the institutions of higher learning in America were . . . established for the purpose of providing the religious leaders of the growing nation with a suitable intellectual background," to the present. In the early days "faculties were selected with reference to ecclesiastical, and even narrow sectarian, requirements long after the theological schools had expanded into colleges and universities," and the effect of this state of affairs is felt in some departments of these institutions even to the present time. In spite of the obvious defects of such a system, it was contended in the paper that there was more sincere devotion to truth than we usually admit. With this situation, various undesirable ones now existing in America were contrasted, and numerous conditions tending toward the lowering of standards and the making of educators themselves into mere employees hired to teach just what they are told to were graphically, and probably even too pessimistically, described. From these undesirable conditions there seems to be no escape other than to make the educators in different lines themselves the determiners of standards in their own fields. The situation in psychology in many Southern institutions, as revealed by the Committee on the Teaching of Psychology, was taken as an example. It had been shown that a very large proportion of the teachers of psychology in the smaller colleges were totally unqualified in training for this line of work. The Southern Society therefore found it necessary to define standards as to qualifications for teaching psychology, both elementary and advanced, and as a consequence the Society publishes now annually a list of institutions equipped with properly qualified instructors, adequate library and laboratory facilities, etc.

to train students either in elementary or in advanced work in psychology. This list is published and distributed to over 420 Southern institutions and also to leading American universities so that students unprepared to do satisfactory work may be required by psychologists, when such students transfer to better equipped institutions, to get the necessary training as a prerequisite to higher courses, even if they must repeat courses already taken. This method has also been taken up by the philosophers in the South, and it is suggested that a similar plan should be carried out by specialists in all the subjects of the curriculum. If this is done, no institution can long continue to maintain in any department of instruction a teacher who is discredited by his colleagues, whatever his local popularity gained by methods of cheap competition and self-advertising may be. "The objection that the plan proposed for standardizing teaching ignores the fact that an instructor's fellow-specialists would not be able to judge concerning this teaching ability would not be valid, since the only purpose of the plan is to determine whether the incumbent of a given position is fit to teach the subject, granted that he actually does have teaching ability." The plan was not offered as a panacea, but it would put into the hands of educators along the several special lines a means of considerable effectiveness in opposing some of the degenerative influences now at work in our college life, and in guarding the rights of students in their pursuit of knowledge and scientific training.

Max Meyer (University of Missouri) gave the closing paper of the afternoon session. He divided theories as to cochlear activity into two general classes—those for which elasticity of some structures in the inner ear is indispensable and those for which such elasticity is irrelevant or else of decidedly secondary importance. Helmholtz, he pointed out, did not originate the theory of stretched strings, this having been first suggested by Cutugno (or Cutuño) in 1780. He also pointed out that Bonnier had first suggested a theory of the second type. His own theory, of course, falls in this second class and was called the Bonnier-Meyer theory. Meyer criticized certain writers for holding that his own theory assumes that a traveling bulge proceeds up the cochlea. The operation of the basilar membrane according to his theory is shown by a model which he had prepared and exhibited in the Ohio meeting of the American Psychological Association but which he did not now have with him. In the discussion it was suggested by Peterson that the large, slow-operating model does not prove that the basilar membrane in the ear operates similarly, and that it would be well to construct a very small model after the order of the cochlea and test it on relatively high frequencies with a device for photographing at very short intervals the different positions assumed by the 'basilar membrane.' In the discussion Meyer made the remark, not published so far as the reviewer knows, that the cochlea functions differently for high than for low tones.

At the close of this session the members of the Society and others present were pleasantly entertained at tea by President and Mrs. McVey in their home.

J. C. Barnes (Maryville College) reported results of an application of the six Seashore music tests to 200 college students. The average score secured was equal to Seashore's 39 percentile, and only on two tests did the Maryville group reach a higher median than that given in the norms. "Very little" correlation was found between music test and intelligence test scores, but the former gave be published in full, was well thought out. Objective psychology was defined as "one of the sciences of human nature which, from the existential and the genetic point of view, deals with the concrete ways in which human beings are impressed by, and respond to, their physical and social environment." The objectives of such a science are "to determine as accurately and fully as possible (1) what kinds of impressions human beings receive from the external world, from other human beings, and from their own internal activities; (2) how these impressions are influenced by such factors as heredity, physiological structures and functions, growth and learning, and peculiar external and internal conditions; (3) what kinds of responses human beings are capable of, how they come about, and how they are influenced by the same factors of heredity, physiological structures and functions, growth, learning, and peculiar external and internal conditions; (4) what general laws and principles seem to underlie these various facts; and (5) what applications can be made of the knowledge of these facts and laws to various fields of practical human endeavor." The author criticised behaviorism for attempting to substitute for the concrete or subjective facts of mental life an empty abstraction of behavior without consciousness, for reducing thereby psychology to a subdivision of comparative physiology, and for being inconsistent in its mechanistic approach to its subject-matter by recognizing 'social' behavior and 'social'relationships, although the term 'social' can have significance only for an individual who is conscious of his fellow beings, who has definite feelings and evaluating attitudes towards them, and who interprets their behavior in terms of his own consciousness and behavior. He warned against making measurement an end in itself instead of a means to an end. Surely, even in this deliberate paper, attempting an impartial evaluation of a *somewhat* new movement in psychology, the author reflects a grotesque and extreme notion, probably arrived at by a gradual and 'unconscious' development in a peculiar atmosphere. It is unfortunate in some respects for any science that such group attitudes should reach extreme stages affording only part-vision. The reviewer who has grown up with American behaviorism, though probably not *on* it, and who has more or less consistently thought of himself as behavioristically inclined does not recognize—except in extreme statements of individuals in their extreme moments—the school of investigators thus described. All the behaviorists whom he knows seem to be human, to be attentive to others' expressions of discomforts—if not directly to their discomforts—to their pleasures, etc., and they appear to differ among one another and themselves concerning several of the views (for they *have* views) ascribed alike to all of them, or to the bugbear behavior*ism*. They do not seem to believe in a mere existential reality in terms solely of visual patterns in a tridimensional reality. Most of them could not follow the author in his apparent dualism; many would not agree that they deny conscious experiences, though they would not accept conscious*ness* as a stimulus to or a regulator of activity; all recognize social relationships both in their social and professional lives and they enjoy friendships alike both with behaviorists and non- or even anti-behaviorists. They could not be quite sure as to what the author meant by the *external* world and could not agree among themselves as to its definition. They recognize and attempt to measure 'evaluating attitudes,' and probably all of them are still using measurements only as means to the end of advancing human life even in its finer relationships.

In such work they sometimes, indeed often, are compelled to go beyond the third dimension, so they cannot all be purely visually minded. They probably all see a distinction between psychology—or 'behaviorism' if one prefers that term—and comparative physiology, and believe in checking their results in research by as objectively consistent methods as possible.

The following new officers were elected in the business metting: Max F. Meyer (University of Missouri), president; H. C. Sanborn (Vanderbilt University) and Anna Forbes Liddell (Florida State College for Women), council members. The retiring president becomes also a council member. The invitations from Peabody College and Vanderbilt University for the Society to meet next spring in Nashville were accepted.

Peabody College JOSEPH PETERSON

THE FOURTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The fourth annual meeting of this association was held on May 10 and 11 at the Illinois Union, University of Illinois, Urbana. There were five sessions, the first four being devoted to the presentation of formal papers each one of which was held open for discussion for a maximum of fifteen minutes. The papers of each session are summarized separately.

During the Friday afternoon session over which Dr. H. M. Johnson (Mellon Inst.) presided the following papers were offered.

H. Klüver (Institute Juvenile Research) described the visual effects produced by mescal. The results produced were related to work done on after-images and eidetic images. Characteristic confusions of an hallucinatory sort were noted and described in detail.

C. A. Ruckmick (Iowa) then discussed "emotions in terms of the galvanometric technique." The point was made that certain bodily conditions like fatigue should be carefully studied before the results could be unequivocally interpreted. Tentative conclusions so far accepted were that a characteristically emotional curve can be detected and that its amplitude is proportional to the intensity of the emotion produced. Improvements in technique and in electrodal connections were illustrated.

M. F. Meyer (Missouri) proposed a "new interpretation of the mechanism of accommodation in the eye." The musculature operates in such a way that the lens is squeezed out through the iris opening producing a marked bulge on its anterior surface. This is due to the fact that the vitreous humor is incompressible and confined. The only way in which lens could find relief under lateral stress is forwards.

H. R. DeSilva and S. H. Bartley (Kansas) examined the effect of decreasing brightness in binocular perception and reported that the subtractive effect with decrease of light intensity as reported by *O* is not in direct proportion to the physical event. It is decreased 18% when the combined light intensity to both eyes is reduced 1/32. When the light to one eye is entirely shut off making a reduction of 1/2 it is apparently reduced only 25% over normal vision.

Miss Mary A. M. Lee (Minnesota) closed this session with a discussion of "the reliability and interrelation of the knee jerk and measurements of steadi-

In the afternoon session over which Dr. R. M. Elliott presided there were a number of miscellaneous papers mainly covering performances under objective tests.

F. Fearing (Northwestern) read a paper on "the organization, content and objectives of the elementary laboratory course in psychology." Under the assumption that the elementary laboratory course is the fundamental introduction to experimental psychology the author advocated more specific attention to experiments which would elicit the interest of elementary students. The main claim of the laboratory is in the direction of a thorough-going *knowledge* of the subject-matter of psychology obtained first hand from experimental procedures. *Information about* psychology on the other hand can more readily be gleaned from the lecture and class room. A very intensive and somewhat heated discussion followed setting forth various points of view in regard to the fundamental aims of the laboratory course.

W. C. Olson (Minnesota) urged a procedure of "measurement by direct observation" in connection with the study of animals and children. Nervous habits and disorders in the conduct of children furnished the material which was observed by this method. A series of charts demonstrated the advantages obtained by the use of a continuous period of observation as over against the dichotomous type of observation which involves the noting of the presence or absence of a particular type described.

Miss Minnie L. Stechkel (Chicago) read a paper on "intelligence and order of birth in family." The main contention statistically approved was that later born children show a higher degree of intelligence than the older children. But in the discussion it developed that this may be accounted for by the fact that young children learn more readily from other and older children than from their parents and also the possibility of the parents becoming more expert in the treatment of their children as the parents learn by experience.

J. E. Anderson (Minnesota) by means of numerous charts presented an important series of studies made on the "language development in twins." A number of elements were analyzed out with the result that with respect to all criteria of language development twins are retarded in comparison with single children. This retardation increases rather than decreases within the age range studied. Ego-centric responses are lower, however, in twins than in single children while emotionally toned responses are higher. Correlated with these results is the significant point that the mental age of twins is significantly lower than that of the standard group of single children (singletons).

L. Ackerson(Institute Juvenile Research) enumerated the factors involved in the "relation of children's behavior problems to chronological age and intelligence quotient." 5000 children were given a complete physical, psychological, psychiatric, social examination. The greatest number of conduct problems occurred between the ages of nine and twelve. For girls there was a steady increase until about the seventeenth year. There was a maximal number of both conduct and personality problems for the adolescent who showed an I.Q. between 70 and 99.

Miss Mary Shirlye (Minnesota) read the next paper on "the irritability of infants at test situations during the first year." By repeatedly observing infants during the first year of life and giving them both physical, anthropometric and psychological examinations it was noted that the higher scores for irritability occurred during the anthropometric examination. An average score was obtained during the physical and the lowest score during the psychological tests. There was, in general, a decrease in irritability with the age of the infant.

Miss Florence L. Goodenough (Minnesota) next described "an observational study of anger in young children." The records were chiefly kept by the mothers of 24 children ranging in age from eight months to seven and a half years giving a total of 1619 daily observations and a total number of outbursts of anger of 1350. The reports show much marked diurnal variation in the frequency of outbursts and much individual variation. There is some causal variation shown in the fact that the individual differences were highly correlated with the method of control by the parents.

C. L. Hull (Wisconsin) introduced the last demonstration made by a physical chemist which attempted to show that various laws concerning the conditioned reflex familiarly exhibited in the Pavlov dog could be produced by a device actuated by electrical and chemical means. The demonstration was unique and ingenious. Similar mechanical arrangements were promised for the higher thought processes!

On Saturday evening after the annual dinner at Newman Hall the presidential address on the "Measurement of Infant Behavior" was read by A. P. Weiss (Ohio State). The paper which was enthusiastically received outlined the results obtained in the laboratory at Ohio State under very carefully controlled stimulus conditions. The theoretical importance of such studies for the psychology of human behavior was outlined. In many respects it presented an unparalleled account of the early movements and characteristic responses of the human infant.

The banquet was attended by 125 psychologists and guests. At the close of the address Dr. Max F. Meyer received the highest vote for presidency and Dr. Horace B. English was added to the Council in place of the retiring member Dr. Samuel Renshaw. The next meeting of the Association will be held at Antioch College, Yellow Springs, Ohio at a date later to be determined by the Council. Among the institutions represented by the 170 individuals who were officially registered at the meeting are the following: Chicago, Drake, Hamline, Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Northwestern, Purdue, Ohio, Ohio State, Washington, Wisconsin, Antioch, Chicago Normal, Grinnell, Iowa State College, Ohio Wesleyan, Stevens, Wittenberg, Bradley Institute, Institute for Juvenile Research, Lewis Institute, Mellon Institute.

Opportunity was afforded for visting the laboratories at the University of Illinois and the work done by Dr. C. R. Griffith of the Athletic Association in a very well-equipped laboratory in the new gymnasium. There was also an apparatus exhibit by Stoelting and others. A vote of thanks was given to the University and to the local department for the splendid arrangements made and accommodations afforded. The meetings were the best so far held in point of attendance, quality of papers read, and amount of discussion usually aroused.

University of Iowa CHRISTIAN A. RUCKMICK

THE TRAINED OBSERVER IN AFFECTIVE PSYCHOLOGY

In a note[1] dealing with trained observers in affective psychology Dr. J. P. Nafe writes: "I agree with Young that previous training is important in determining the *type* of report to be expected but I do not agree that it is even a factor in determining *the report*, that is, the facts reported." I doubt whether the distinction between "type of report" and "facts reported" can be maintained. In the laboratory we are obliged to accept as a starting point the report as it is given by the observer. The training of the observer may determine whether this or that aspect of experience is attentively clear, whether this or that pattern will be observed, whether the experience means this or that to the observer. Training may determine whether the observer reports "P is a non-clear affective process," "P is a bright pressure," or "P is a bodily reaction." The education of the *O*, his theoretical views, his understanding of words and habits of speech, the suggestions which happen to reach him determine not only the type of report but to a large extent the "facts reported."

There are uniformities within experience which are relatively independent of the attitude of the observer and which depend directly upon uniformities of conditions. The practical question arises: How can uniformities be *discovered* in the subject matter of affective psychology when it is clear that uniformities may also be *made* by the observers' training?

In one respect Dr. Nafe's note does not accurately present the position I had hoped to convey. I do not know how he gained the impression that I held that training is "prejudicial rather than effective in discovering the facts," and that the functional report in the form "I don't like it" has a "superior value," for I nowhere make that claim. My point is that training to a very large degree *makes* the facts of observation as well as the type of report. The question of relative value has not been discussed.

University of Illinois P. T. YOUNG

REVUE DE PSYCHOLOGIE CONCRÈTE

The first number of the *Revue de psychologie concrète*, dated February 1, 1929, has been received. The *Revue*, edited by C. Politzer, is to be international in its scope and the exclusive medium of the 'new' psychology. It aims, we are informed in a prefatory editorial, at two things: to unify the criticisms made of classical psychology—the psychology that historically had its origin in philosophy, physiology and biology; and the foundations of the 'new' psychology—psychoanalysis, individual and technological psychology, and characterology. Five departments are planned: foundations of psychology; positive research; documentary studies; permanent problems; and reviews and bibliography. Only four are represented in the first number—positive research being omitted. The first number contains the following articles: C. Politzer, Psychologie mythologique et psychologie scientifique; F. Giese, Théorie et pratique en matiére de psychologie; J. Kantor, La psychologie organique; A. Adler, Les idées fondamentales de la psychologie individuelle; A. Hesnard and E. Pichon, Aperçu historique du mouvement psychanalytique français; C. S. Myers, La psychologie industrielle en Grande-Bretagne; C. Politzer, La psychologie générale et la psychotechnique: Texte de l'enquête; H. Prinzhorn, La crise de la psychanalyse; and a critical note and book review by the editor. K. M. D.

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[1] J. P. Nafe, Dr. Young on trained observers, this JOURNAL, 41, 1929, 161-2.

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A QUANTITATIVE CONSIDERATION OF THE PURKINJE PHENOMENON

By Wm. T. M. Forbes, Cornell University

It is well known that the relative brightness of the various colors of the spectrum at constant energy, which is fundamental in all photometric work, varies with the brightness of the stimulus light to which the eye is adapted. The following is an attempt to derive from the available data (mainly from an experiment of König's)[1] the law of this variation, so that it will be possible to correct for the varying brightnesses under which experiments on color have been made. Success has been far from complete, the available authorities being far from consistent and subject to various sources of error; but it is believed that the coefficients given below approximate the true values.

If we adopt a photochemical explanation of the sensitiveness of the eye to brightness, as is generally done, the curve of brightness should be the sum of a larger or smaller number of curves, severally representing the photochemical activity (and presumably the absorption) of the various sensitive substances in the eye. These curves should each be of constant form and position,[2] but

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*Accepted for publication February 6, 1929.

[1] A. König, *Collected Papers*, 1903, 144-213.

[2] It is well known that the absorption bands of pigment solutions tend to change in form with increasing concentration, generally widening, and often unsymmetrically; but in the present case, if we assume (as is generally done) that adaptation is a matter of the decomposition of the pigment by light, and that the amount of undecomposed pigment is in inverse proportion to the illumination to which the retina is adapted, the actual concentration with usual lighting must be very small. With such concentrations the change of absorption-curve with concentration can be neglected. Only in the actual threshold case may it be necessary to consider the matter. The most frequent effect of increasing concentration seems to be to flatten the apex of the absorption curve, and this effect can perhaps be seen in some curves of the absorption of the visual purple, which was so concentrated that on occasion more than half of the light of the maximal wave-lengths was absorbed.

TABLE I

König's Coefficients

Reduced to sunlight and normal spectrum, using König's correction methods; also reduced to approximately unit sum

	K. stage	H	G	F	E	D	C	B	A	S
Normal Observer (König)	K. units	590	148	36.9	9.22	2.30	.575	.036	.00225	.00024
	Photons	295	74	18.5	4.61	1.15	.288	.018	.0011	.00012
	670 (mμ)	.0089	.0085	.0075	.0058	.0025	.00076	.00037	.00005	.000035
	650	.0348	.0268	.0221	.0160	.0084	.0026	.0011	.00017	.00008
	625	.0779	.0731	.0581	.0490	.0249	.0081	.0037	.0011	.0008
	605	.1166	.1101	.0999	.0766	.0408	.0163	.0076	.0038	.0026
	590	.1294	.1231	.1142	.0883	.0603	.0243	.0159	.0103	.0075
	575	.1330	.1227	.1264	.1048	.0723	.0419	.0343	.0293	.0202
	555	.1468	.1431	.1402	.1334	.1100	.0883	.0808	.0728	.0612
	535	.1340	.1334	.1349	.1451	.1490	.1419	.1372	.1388	.1328
	520	.1066	.1088	.1101	.1222	.1534	.1751	.1756	.1834	.1780
	505	.0674	.0738	.0813	.1171	.1451	.1759	.1935	.1974	.1935
	490	.0565	.0492	.0511	.0609	.1017	.1428	.1507	.1560	.1902
	470	—	.0281	.0336	.0400	.0773	.1044	.1080	.1222	.1267
	450	—	—	.0213	.0252	.0364	.0583	.0650	.0643	.0688
	430	—	—	—	.0162	.0182	.0200	.0267	.0203	.0170
	K. stage	H	G	F	E	D	C	B	A	S
Protanope (Ritter)	K. units	878	220	54.9	13.7	3.43	.857	.0536	.00335	.00037
	Photons	439	110	27.5	6.8	1.71	.428	.0268	.00168	.00018
	670 (mμ)		.0007		.00048	.00037	.00014	.00006	.00003	.00004
	650		.0029		.0020	.0015	.00082	.00019	.00012	.00012
	625		.0140		.0110	.0077	.0031	.0011	.0010	.00098
	605		.0400		.0323	.0222	.0102	.0046	.0040	.0022
	590		.0744		.0662	.0485	.0221	.0116	.0090	.0068
	575		.1142		.1018	.0877	.0396	.0256	.0223	.0238
	555		.1534		.1498	.1410	.0832	.0664	.0604	.0677
	535		.1682		.1657	.1647	.1461	.1336	.1208	.1302
	520		.1483		.1551	.1636	.1897	.1766	.1661	.1852
	505		.1186		.1302	.1648	.1908	.2013	.2058	.1580
	490		.0860		.0880	.1046	.1536	.1723	.1750	.2025
	470		.0492		.0596	.0688	.1085	.1231	.1353	.1026
	450		.0223		.0283	.0355	.0383	.0592	.0781	.0952
	430		—		.0102	.0123	.0134	.0184	.0222	.0245

probability curve, and the absorption curve of the visual purple should either be of a similar form, or show wider bands embracing the bands of activity.

The obvious first assumption is that the pure scotopic curve, as shown for instance by the threshold brightness for the various wave-lengths, should be represented by the most sensitive substance alone. As a first approach to a solution, König's curves[5] (Fig. 2, plotted points) corrected for the prismatic spectrum and for the color of the source[6] were taken (see Table I) and from each

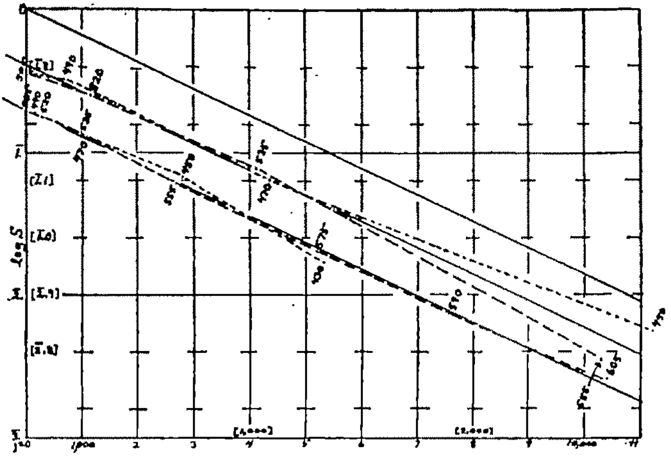

FIG. 3. FITTING OF GAUSSIAN TO KÖNIG'S S CURVE
The logarithm of the *Helligskeitswerthe* is plotted to the square of $\lambda - c$, c being taken as 503 mμ. The bracketed scale-values apply to the middle curve, and the upper line represents the Gaussian with maximal ordinate = 1.

$$[S = e^{-k(\lambda - c)^2}]$$

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[5] König, *loc. cit.*, 168.

[6] König, *loc. cit.*, 225. I have reduced König's data to normal spectrum and sunlight by multiplying by the factors he himself gives. These are based on his own measurements of his own prism and a direct comparison with sunlight brought into his apparatus. They are probably not strictly equivalent to the modern 'standard noon sun'; but the divergence is probably within the range of uncertainty of the whole calculation. If the temperature was not exactly that for which the comparison with the sun was made, the uncertainty introduced may be larger, and this may explain in part at least the course of the blue end of König's curves, as a small difference in temperature causes a relatively large change in the amount of blue light. The yellow spot has not been allowed for in any of these data. It probably causes the steepness of the curve at about 520 mμ and the lack of fit in that region. It will also give the scotopic and blue coefficients a value lower than will truly represent the process in the retina itself, but a value which fairly represents their effective contribution to vision.

the coefficients in the resulting sets of probability equations have to be calculated for each curve. The coefficients of the three probability curves (Fig. 1) and their luminosities for the various wave-lengths are given in Tables IIA and IIB.

TABLE IIB

COEFFICIENTS OF THE ELEMENTARY PROCESSES
For unit sum at 10 mμ intervals from 400–700 mμ

λ	P_1	P_2	S
690 (mμ)	.0023	0	0
680	.0057	0	0
670	.0125	.0002	0
660	.0245	.0006	0
650	.0429	.0014	0
640	.0671	.0032	0
630	.0939	.0066	.0001
620	.1178	.0129	.0004
610	.1316	.0227	.0009
600	.1316	.0369	.0021
590	.1178	.0550	.0047
580	.0939	.0751	.0093
570	.0671	.0946	.0173
560	.0429	.1092	.0292
550	.0245	.1159	.0455
540	.0125	.1131	.0649
530	.0057	.1013	.0850
520	.0023	.0831	.1029
510	.0008	.0629	.1138
500	.0004	.0437	.1156
490	.0001	.0278	.1082
480	0	.0163	.0926
470	0	.0088	.0731
460	0	.0043	.0530
450	0	.0020	.0352
440	0	.0008	.0215
430	0	.0003	.0121
420	0	.0001	.0058
410	0	0	.0029
400	0	0	.0015
c	605 mμ	548 mμ	503 mμ
k	.00056	.000425	.000425
multiplier	.1336	.1162	.1162

The calculated percentages of the three processes at König's experimental brightness levels are given also in Table III.

If we plot the resulting curves (Fig. 2) with the corresponding König data (plotted points), we find that there is a fair fit, but that the marked separation of the modes at 605 and 548 mμ is partly smoothed. Still we may say that the curves fairly represent the König data. On the other hand, it will be seen that in the case of all the recent data except Sloan's, none of which was taken over a range of luminosities sufficient to give any independent check of the law of variation with brightness, there is about an equally good fit, but the errors are of the opposite sign, the separation between the modes being much too deep—for these modern curves show hardly a trace of the depression about 575 mμ.

TABLE III

Proportions of the Elementary Processes
Fitted to König's data by the method of least squares

Brightness		Proportions			Proportions ÷ Brightness		
König	Photons (approx.)	P_1	P_2	S	P_1	P_2	S
S	.00012	0	0	1.	0	0	2000.
A	.0011	0	.092	.92	0	20.4	204.
B	.018	0	.135	.855	0	1.875	11.8
C	.288	.006	.201	.78	.005	.175	.678
D	1.15	.088	.362	.538	.019	.0805	.117
E	4.61	.185	.535	.272	.010	.0291	.0148
F	18.5	.27	.57	.16	.00365	.0077	.00243
G	74.	.30	.575	.1115	.0011	.00195	.000378
H	295.	.33	.57	.115	.00028	.00048	.0001
		(Protanope)					
S	.00018	—	0	1.			
A	.00168	—	0	1.			
B	.0268	—	.04	.96			
C	.428	—	.175	.85			
D	1.71	—	.555	.455			
E	6.8	—	.64	.36			
F-H	27-440	—	.79	.16			
		(Deuteranope)*					
H	336	.26	.67	.06			

*Rough calculation, rejecting the point at 520 mμ.

To test the inverse relation between brightness of stimulus and concentration of pigment, we may divide each coefficient by the absolute total brightness of its level, and plot the resulting figures (Table III, columns 5–7) on a logarithmic scale. In this case the curve of total brightness plotted against total activity is of course a straight line, and the curves of the individual components should approximate to straight lines (Fig. 5). It is obvious that this relation is approximately true, but that some (though its existence and approximate character are required by the data), as it does not appear in any approach to isolation either in the sensitivity curves, even at the highest brightnesses,[13] in any color-blind cases, or in the absorption spectra of visual pigment. Nevertheless, a glance at the series of curves in Fig. 2 will show that some such process is necessary to explain the manner in which the left side of the curve gradually rises with increasing brightness. It is interesting to compare this process with the 'red' process of the Young-Helmholtz theory. The two are closely similar in position, but there seems to be a definite divergence. The process, as I identify it, has a maximum at about 605 mμ,[14] and with an illumination of about 50 photons contributes about 25% of the brightness sensation; the formulation usual on the Young-Helmholtz basis uses a maximum at below 590 mμ, and covers a substantially wider range of wave-

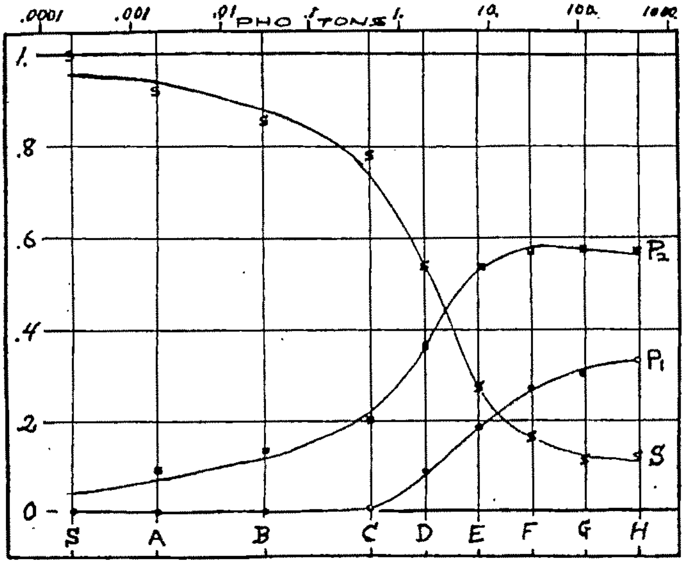

FIG. 6. EXCITATIONS PLOTTED FOR INTERPOLATION

lengths. Judd's calculation on this basis would indicate that the process contributes 45% of the brightness. This divergence is well beyond the probable range of uncertainty of my curve.

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[13] L. L. Sloan (*Psychol. Monog.*, 38, 1928, no. 173) found that the red process was relatively very highly sensitive to dim light in the photopic eye, as shown by the next to bottom row of graphs on her Fig. 8, facing p. 68. As equilibrium approached the red process dropped to its usual position of subordination.

[14] Even higher in Sloan's foveal runs.

The second photopic factor (P_2). The position of the maximum of the second photopic factor (P_2) is fixed by the convergence of data from color-blind (protanopic) cases as well as the analysis given above. In the protanope, the first process is evidently negligibly small or absent, and the long-wave side of the second process is laid bare. In fact the protanopic case which König analyzed with some completeness can be matched with fair accuracy by a system of two components (Fig. 7). It is interesting to note that the absorption curve of *fish* visual pigment (Fig. 9) as given by Köttgen and Abelsdorff, shows a maximum at about the same point, and that attempts to bleach it differentially with colored light failed to show any indication of heterogeneity; so it seems as if the fish had a single pigment corresponding to this process. The main difficulty is that the absorption band of the fish visual pigment is much wider than would fit König's data (Fig. 9). König found one observer[15] who had a curve in the red-green region fairly representing this process alone, and who was night-blind, therefore presumably lacking the scotopic substance; but the blue region was not investigated in his case.

This curve corresponds closely to the 'green' curve under the Helmholtz theory. A fair approximation is necessitated by the data; but the exactness of fit is due to the fact that the present curve was derived on the assumption that the protanope has only two processes contributing to the sensation of brightness. In fact the residuals in the normal *O* (Fig. 2, top) show a systematic drift which indicates that this second photopic curve should be somewhat narrower and farther toward the short waves, and that the protanope measured by König should be allowed a small proportion of the first photopic process.

A third photopic factor (P_3). The approximate coincidence of the two preceding processes with the red and green processes demanded by the Young-Helmholtz theory, suggests a search for a third, a blue, factor. In fact a study of the residuals (Fig. 2, top) seems to show a hump centering at about 450 mμ, the presumable position of such a third process. The light used by

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[15] König, *loc. cit.*, 332.

König was weak in the blue, and it was not possible for him to carry his brighter runs to the blue end; with the dimmer lights the process is certainly weak in proportion to the scotopic process which overlies it, and the observational values are so ragged that no definite conclusion is possible. The only modern data over any considerable range of luminosities (Sloan's) likewise show but little evidence of this process, though in other recent data, where the scotopic factor is eliminated by using a stimulus area more nearly limited to the fovea, a small mode is visible, but even

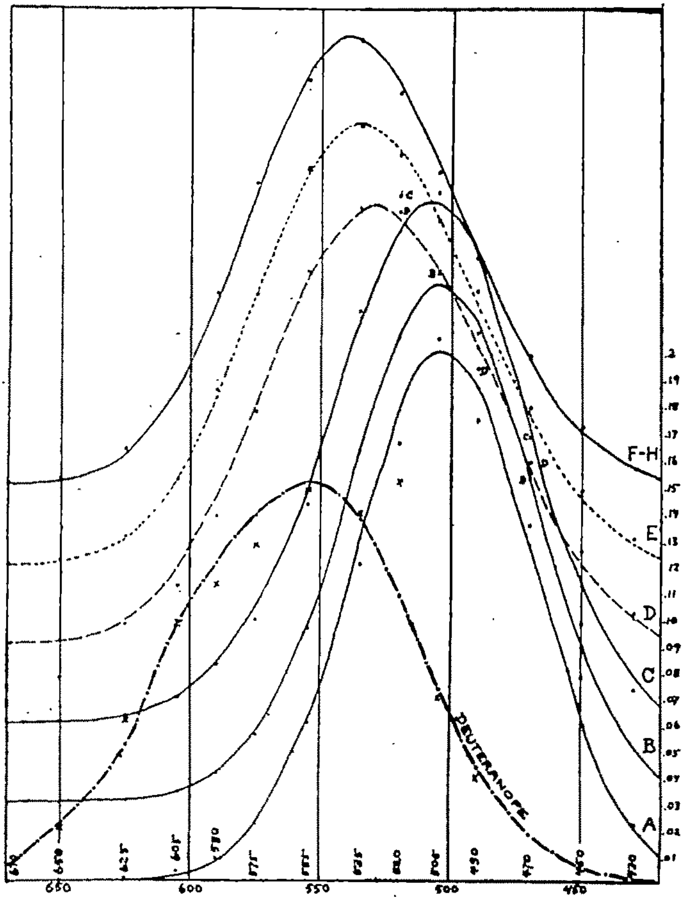

FIG. 7. KÖNIG'S HELLIGSKEITSWERTHE FOR COLOR-BLIND *Os*: PROTANOPE (RITTER) AND DEUTERANOPE (BRODHUN), WITH FITTED CURVES

smaller than König's curves would lead one to expect. If the process is present it would seem to have a mode at about 450 mμ and a coefficient (P_3) between 0.05 and 0.1 from König's data, or a coefficient of about 0.02 according to more recent figures. The figure shows no definite drift with changing adaptation, unlike the other factors, and may be an artifact, caused by some moderate error in the factors used in reducing the observational values (based on artificial light) to sunlight. If the observational light were a little whiter than supposed, it would produce some such distortion of the curve.

Color-blindness: (*a*) *Protanopic.* König has studied a single protanopic *O* (Ritter) over the whole range of brightness that he used. His data, when corrected for the prism and the color of the source used (see Table I), can be matched with the use of only two of the three processes necessary for a normal *O*, even more closely then the three processes represent the normal *O*. The coefficients do not run so smoothly as in the normal case, the Purkinje shift taking place mainly between König's levels C and D, *i.e.* about 0.4 and 1.75 photons (his units being apparently about 0.5 photons), and there being no further shift reported by König above about 25 photons, where in the normal observer the shift is largely due to the change in the proportion of the two photopic processes. (See Fig. 7.)

The calculated coefficients are as follows.

Brightness		Proportions		
König designation	Photons (approx.)	P_1	P_2	S
A	0.002	0	0.00	1.00
B	0.027	0	0.04	0.96
C	0.43	0	0.175	0.85
D	1.72	0	0.555	0.455
E	6.90	0	0.64	0.36
F-H	28. – 425.	0	0.79	0.16

As actually calculated, a few of the coefficients for P_1 have small negative values, showing that our P_2 is a little too wide, even on the assumption that P_1 is wholly absent in the protanopic case.

For the protanope, then, the effect is as if the red process had been eliminated from the equation of brightness, as well as from the color sensation. If this case stood alone, there would seem to be very definite evidence that the color and brightness processes were identical. On the other hand, the deuteranope shows a very different situation.

(*b*) *Deuteranopic.* König gives one definite set of quantitative data on a deuteranope, and Gibson and Tyndall have measured a second *O*,[16] who from the incomplete data presented is either a deuteranope or an extreme deuteranomalous case. König examined his *O* at two levels only; the first level, A, showed as is to be expected only the scotopic process in its usual form, and is of no special significance. His brightest level (H), however, is very striking (Fig. 7, dot-and-dash curve). Instead of being a simple sum of two processes, as in the case of the protanope, the curve is essentially the normal curve, though with a slightly low value for S and high value for P_1. The second photopic process, P_2, which should be wholly absent if it represented merely the green process, is not only present, but produces the maximal point of the curve; while the process P_1 (red), which should be dominant, merely produces the usual well-marked hump on the long-wave side of P_2. In fact there is *no* simple curve which can be fitted to the photopic portion of this curve, while by assuming the same two processes present as in the normal *O* the fit is satisfactory. In this case, therefore, we have only *two* color processes on the Helmholtz theory, or one on the Hering theory, as the color blindness shows, while the brightness curve shows as plainly as possible the separate modes for 'red' and 'green.'[17] This seems to point strongly to a situation where there has been a de-differentiation of the color-process, without any disturbance of the brightness process. The simplest explanation on a Helmholtz basis would be that the red and green processes were present as usual, but had no separate connection with the brain—adding their respective brightnesses, but producing only a single color sensation. On the Hering basis it would indicate the presence of all the normal pigments in the white-organs, but the absence of the red-green system of color-organs, while the protanope would have lost one of the pigments as well as the same set of color-organs. If that were the explanation the reverse defect might also occur, with the color-system normal, but with a brightness curve similar to that of König's *O* (Ritter). No such case appears among the relatively few for which I have seen quantitative data; they would be much more easily overlooked than a color-blind individual, but if at all common should have appeared among Crittenden and Richtmyer's or Gibson and Tyndall's cases.[18] The second possibly deuteranopic case is Gibson and Tyndall's *O* 23.[19] This *O* is not fully described, and I understand was not fully tested, but he is reported as making the usual color-blind confusions, and as his brightness sensation is not defective at the red end must have been a close approximation to a deuteranope. In his case also the coefficients are similar to those of a normal *O* at the same brightness levels, in fact the curve falls well within the experimenters' normal group. As the brightness was not held constant, and appears to lie in a region where the Purkinje shift is rapid, the actual figures of the three processes are of little significance. Still the shoulder on the curve at 600 mμ and the location of the principal mode are just as in König's case.

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[16] K. S. Gibson and E. P. T. Tyndall, Visibility of radiant energy, *Sci. Pap. U. S. Bur. Stand.*, 19, 1923 (no. 475), 154. They are not clear as to whether this was a deuteranope or an extreme deuteranomalous observer. Judd informs me this *O* was not strictly deuteranopic, but the published data give no hint of ability to distinguish between red and green.

[17] It is obvious that, as we have only data at a single brightness level, it will be entirely possible to fit an *unsymmetrical* curve to the photopic portion of this curve, but the curve would not be even an ordinary skewed one, but would have to be bimodal. This is practically what the ordinary Helmholtzians have done in deriving their 'red' curve from the deuteranopic observer.

Modern Researches

When we come to the modern observations, some difficulties appear in relating them to König's data. With the exception of Sloan's very recent work, which will be separately discussed below, none of the modern work has been done over any considerable range of brightnesses. The greatest range is Ives' set, with a ratio of 100 – 1; and even this set lacks the blue end.

This group of experiments as a whole stands in contrast to König's in showing no real evidence of a scotopic component. In general the brightness has been higher than his, and the field seems to have been smaller than in König's case; but even allowing for these two factors, there seems to be some divergence on this point. There is also a curious disagreement between the methods of flicker photometry and of equality-of-brightness, commented on by

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[18] E. C. Crittenden and F. K. Richtmyer, An 'average eye' for heterochromatic photometry and a comparison of a flicker and an equality-of-brightness photometer, *Sci. Pap. U. S. Bur. Stand.*, 14, 1918, (no. 299), 87-114; Gibson and Tyndall, *loc. cit.* Among this rather large number of subjects there are several with the slightly enhanced red-sensibility characteristic of the deuteranope, but none with the red-defect of the protanope.

[19] Gibson and Tyndall, *loc. cit.*; (cf. Note 16).

Ives. This is presumably due to the psychological invalidity of the flicker photometer, whose function breaks down completely at a level somewhat below that of Ives' observations; but the point calls for further study.[20]

This group of curves also calls as a whole for wider wave-length ranges for the component processes, the relative depression between the two photopic modes (at about 575 mμ) being obscure or even absent. There was considerable variety in comparison lights, but so far as I know none duplicated König's use of a monochromatic comparison light.[21]

Ives published two series of data,[22] the first obtained by both the flicker and equality of brightness methods over a range of brightnesses, and the second limited to flicker data at the brightest level, with a small field.

His data are defective at the blue end, and so cannot be used as evidence in the matter of a separate blue process. The earlier set, which used a dim light (about 0.25 meter-candles), lacks even the region where a scotopic process might be revealed; but the half-curve which they show (from the red to the green) is very interesting as indicating a reversed Purkinje shift with the flicker. The cause of this is obscure. It may be in part connected with the sensation of flicker, whose mechanism is unknown and which may involve a time factor and perhaps other factors as well as pure brightness. Also the stimuli used included (as usual) both foveal and parafoveal regions. In this case there seems to be some sort of rivalry between the two, and the judgments of equality may in Ives' case have been made with the fovea, which from the little evidence we have appears to lack the scotopic process, and may be unique in other ways.[23]

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[20] Sloan's observations on more purely foveal stimuli seem to suggest that the behavior with the flicker photometer is that produced by a pure foveal stimulus, while that with the equality of brightness method more nearly represents that due to the parafoveal region. It looks like a case of rivalry between the two zones of the retina. See the last paragraphs of this paper.

There appear to be at least two parafoveal zones differing in scotopic sensibility, besides the fovea. The inner one becomes blind at a level much lower than that at which the fovea becomes night-blind, perhaps a quarter as bright, but at a higher level than the zone of five or ten degrees diameter which may well have been included in König's field.

[21] König used a yellow-green spectral comparison light (535 mμ), which undoubtedly fatigued the retina differentially for component P_2, as well as the light being measured, and probably reduced the coefficients of P_2 to below their true value. Ives used a white (or rather yellow) comparison light and probably got the closest approach to an eye not fatigued for color; Gibson and Tyndall apparently had no comparison light or surrounding field, and the light being measured undoubtedly fatigued their eyes very unequally for the several component processes. It is not clear what effect this would have on their measurements; but these three sets of data are evidently on the whole far from comparable. The ideal device would be to use a white background of fixed brightness to keep the eye in a steady state of adaptation, cutting off this light as briefly as possible while observations were being made. Sloan did this; but her exposure periods (2 sec.) were perhaps too long in the case of the dark-adapted eye, and she did not use adaptation levels near those of the stimulus, but only one brighter and one apparently much dimmer than her whole series of stimuli.

[22] H. E. Ives, *Phil. Mag.*, 24, 1912, 149-188, 845-863.

[23] See the discussion of Sloan's foveal data at the end of this paper.

Ives' second run was at a single brightness. Calculation of the coefficients, on the assumption that the curves extracted from König's data are correct, gives the two upper curves of Fig. 10. There is a fair fit, but the calculated values are too low at 590 mμ and too high at 550, indicating that the curves of the elementary processes are too narrow. Ives' mean curve fits fairly well on the whole, but his personal curve is too steep on the blue side, the effect to be expected from abnormally heavy yellow pigment. The coefficients work out

	P_1	P_2	S
Mean curve	0.22	0.78	0.004
No. 17 (HEI)	0.19	0.81	0.003

The S values are insignificant, of course.

Gibson and Tyndall[24] used the method of 'equality-of-brightness' by small steps. As they used this method it gives the rate of change of brightness (dy/y) at a series of selected points along the visibility curve. The coefficients as published by them are only in part at the points where observations are stated to have been taken, and no statement is made as to how they were interpolated. The method also requires a calculation along from point to point, and some assumption (I am not sure what) of the course of the visibility curve between observed points. Their brightnesses were not held constant and fluctuated over a range of 10 to 1 between the limits of 690 and 450 mμ, and much more at the extreme ends. If we allow that adaptation caused about as much shift as is indicated by König's curves E, F, and G, which seem to cover this range of brightnesses, we can apply corrections. A rough calculation shows that this would not change the general shape of Gibson and Tyndall's curves, but would visibly improve the fit at the red end where their curve fairly matches König's curve E, and much more strikingly at the blue end, where at least half of the irregularity on which they comment seems to be a direct reflection of the irregular changes in brightness. There seems to be a residue, which from its form would suggest a blue process (P_3) of about 2%. As in the other cases, the scotopic component comes out negligibly small. The coefficients work out

	P_1	P_2	S
Average	0.23	0.76	0.01
(EPTT)	0.30	0.70	negative[25]

One of Gibson and Tyndall's *Os* was a deuteranope or substantially red-green weak. It is striking that his curve, while red-strong, falls within the range of Gibson and Tyndall's normal *Os*, showing no substantial defect in the second ('green') photopic process; in fact the curve is a close match to Tyndall's own. The coefficients work out (assuming S as zero) $P_1 = 0.306$, $P_2 = 0.686$, $P_3 =$ about 0.03. P_3 should be reduced on account of the variation of brightness to 0.02 or less and may be illusory.

On the whole, Gibson and Tyndall's curves fit the coefficients rather surprisingly well, except for the extra steepness from 500 to 520 mμ where the yellow pigment causes its usual distortion.

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[24] Gibson and Tyndall; (cf. Note 11).

[25] Note that Tyndall is as red-sensitive as a deuteranope.

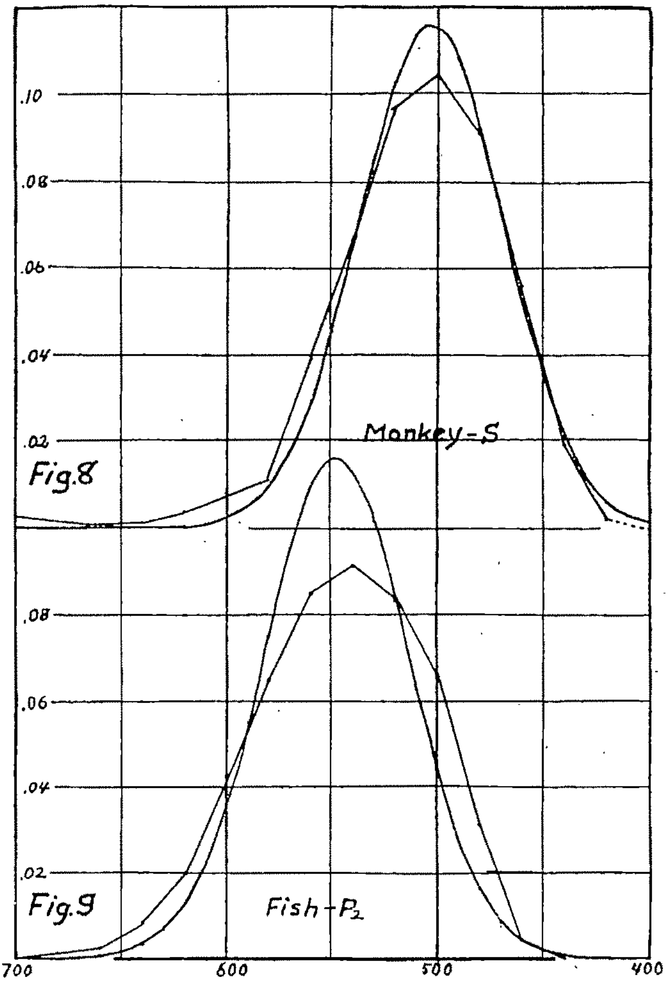

Fig. 8. Curve of Elementary Process S Superposed on Absorption of Visual Purple
(monkey)

Fig. 9. Curve of Elementary Process P_2 Superposed on Absorption of Visual Purple
(fish)

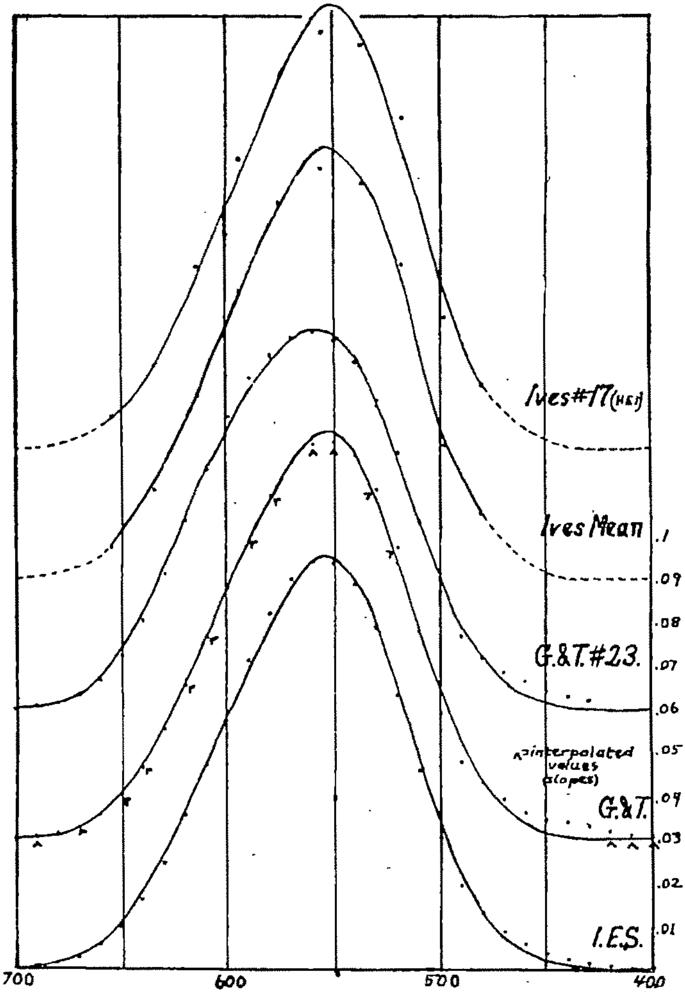

FIG. 10. VARIOUS MODERN DATA ON EXCITATIONS, WITH FITTED CURVES

The Illuminating Engineering Society's curve was a compilation,[26] but I calculate it because it was used as a basis for Judd's calculation.[27] The fit is not quite so good as with Gibson and Tyndall's curve, or for that matter quite so good as Judd's solution. It is too high at 580 mμ, pointing to wider components, as in the other modern curves, and there is the usual small (blue) excess, of less than 2%. The coefficients are $P_1 = 0.23$, $P_2 = 0.77$, $P_3 = 0.015$, and $S = 0$. P_3 is not used in the plotted curve, but estimated from the residuals.

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[26] *J. Opt. Soc. Amer.*, 4, 1920, 58; 6, 1922, 541.

[27] D. B. Judd, *ibid.*, 10, 1925, 635.

Visual pigments. Köttgen and Abelsdorff give a considerable range of data on the extracted visual pigments of the eyes of various vertebrates.[28] If the visual process is primarily a photochemical one in which the visual pigment functions, the pigment which is present in large quantity should be that which serves in the scotopic process. As already noted by various workers (*e.g.* König) the absorption of the visual purple corresponds closely to the scotopic visual process. On plotting Köttgen and Abelsdorff's curves the fit seems very fair, with an allowance of some 3 mμ for the index of refraction of the cones (Fig. 8), but there appear some irregularities at the red and blue ends. The latter (not obvious in the specimen figured) are presumably caused by a slightly incorrect allowance for the yellow tint of the blood mixed with the extracted pigment and the color of the solvent. The red residuals are suggestive in the present connection. In the extraction of the pigment, red or orange light was used. In the latter case there seems to be a definite excess at the red end, possibly indicating that a little of the P_1 pigment had escaped bleaching by the orange light used, while in the case where red light was used the excess is more widely distributed and may well be caused by the survival of some P_2 as well as P_1. The amount of this excess varies a great deal in different determinations by Köttgen and Abelsdorff, as might be expected to result from varying exposures to the red light during the preparation.

Another interesting comparison is of the different pigment found by Köttgen and Abelsdorff (Fig. 9) to be general in the fishes. This matches fairly well with the curve for P_2, and suggests that it is really the pigment of P_2. Attempts to bleach this pigment differentially with colored lights failed, and the fishes presumably have only a single pigment; but there seemed some hint of success in higher animals. Possibly by operating under blue light free from red (the common blue glass transmits some red light) there might be greater success in uncovering the photopic pigments.

After the main part of this paper was finished, Sloan's study of the visual function for various states of adaptation and various stimuli was brought to my attention.[29] So far as I know this is the only paper in the field so far that has attempted to fix both physical and physiological conditions, so that the condition of the eye as well as the stimulus may fairly be said to be known. Only two adaptation levels were used for practically all of the work, one brighter (135 mc. = 260 photons) and the other dimmer than the series of stimuli used. In the case of the bright-adapted eye, the method probably really kept the adaptation level practically constant, as the eye when in a photopic condition adapts very slowly to *decreasing* illumination, and stimulus-times were short. On the other hand, the eye adapts very rapidly under scotopic conditions, and much more rapidly up than down, so that it is not unlikely that in the experiments with the dark-adapted eye, the adaptation was more nearly to the stimulus light than to the (unstated) surrounding field. The

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[28] Köttgen and Abelsdorff, *Zsch. f. Psychol. u. Physiol. d. Sinnesorg.*, 12, 1896, 161.

[29] L. L. Sloan, The effect of intensity of light, state of adaptation of the eye, and size of photometric field on the visibility curve, *Psychol. Monog.*, 38, 1928, (no. 173).

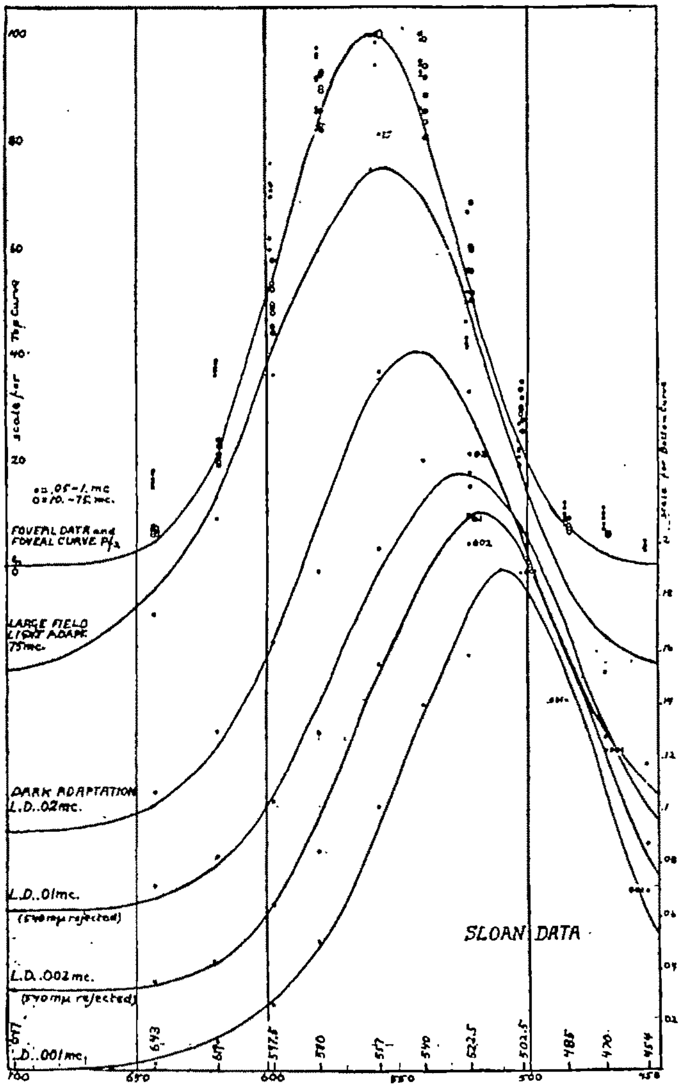

FIG. 11. SLOAN'S DATA WITH FITTED CURVES

The four lower graphs are for the dark-adapted eye with weak stimuli and large (5°) field. The next is of the brightest stimulus used, light-adapted eye and the bright field. The upper curve is the 'green' elementary process, moved 10 mμ toward the long waves, superposed on eight sets of Sloan's values for the fovea (57′ field). The circles are values for bright stimuli and the solid dots for faint stimuli.

lower values as given by Sloan, then, are nearly comparable with König's experiments, and should show a similar course of adaptation in the case of the parafoveal field, as in fact they do (Table V, Fig. 11). The method of equality of brightness was used, and it is interesting to see that the results are more nearly comparable with König's than any other recent work, though on the whole somewhat more irregular. The scotopic values for corresponding illuminations are lower than in König's case, correlated with the smaller field.

TABLE V

SLOAN'S DATA

		Illumination		P_1	P_2	S
Adaptation	mc.	factor	photons			
dark	.001	3.93	.0039	.025	.205	.757
"	.002	3.875	.0078	—	.38	.63 ‡
"	.01	3.66	.0365	.024	.455	.48 ‡
"	.02	3.54	.0708	.048	.726	.198
"	.2	3.09	.618	.186	.790	.023
"	5.	2.39	11.95*	.252	.778	negative
"	10.	2.275	22.75*	.285	.763	"
"	20.	2.16	43.2 *	.221	.820	"
"	40.	2.06	82.4 *	.259	.788	"
light	75.	1.91	143 †	.209	.811	"

*These values are calculated on the basis of adaptation of the pupil to the stimulus light; they should be somewhat increased on account of the exposure of the eye to a dark field between exposures to the stimuli. The factor for a completely dark-adapted eye is about 4.

†Based on the brightness of the surrounding field, 135 mc. equals 258 photons.

‡Omitting the irregular value at 540 mμ.

Sloan's work is the first quantitative work I know of which gives data on the fovea fairly free from the influence of parafoveal regions of the eye.[30] She used a stimulus area of 57′ diameter, which probably included only a very small amount of parafoveal retina.[31] Her results show practically no change of maximal sensitivity with adaptation, and only a very small change in the shape of the curve. Her curves are so nearly identical that I have averaged two groups of eight each to study the elementary processes, in the hope of reducing the accidental error, which seems rather large in the individual curves.

It is definitely impossible to get a good fit with the elementary curves used for the parafoveal region, but if the maximum of each curve is moved about

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[30] Sloan used the natural pupil. In calculating the retinal illumination for comparison with other work, I assume that with the bright adaptation-field the eye was adjusted to the adaptation-field, but that without the adaptation field the iris was in a state corresponding to the stimulus light, or for the brighter stimuli at a somewhat lower level. This is arbitrary but seems the closest approximation possible in the circumstances. I have interpolated the correction factors for size of iris from Schroeder (*Zsch. f. Sinnesphysiol.*, 57, 1926, 201 ff., esp. 213), which is also a source of information for a variety of optical units.

[31] The fovea is a horizontal ellipse of somewhat irregular outline, and its minimal height, projected, is probably less than 57′.

10 mμ to the lower wave-lengths a good fit is possible.[11] It is also necessary to decrease the width of the 'green' curve, at least. The following is a rough estimate of the resulting coefficients;

	Pf_1	Pf_2	Pf_3	Sf
Wave-length maximum (*c*).	617 mμ	560	462	515
k	0.00056	0.000455	0.000455	0.000425
Sum of 8 curves, 0.05–1. mc.	0.14	0.81	0.05	0
Sum of 8 curves, 10–75 mc.	0	0.94	0	0.06

This result seems wholly anomalous, but is much like Ives' results with the flicker photometer. It seems to indicate that whatever is the mechanism by which the eye judges brightness, in any case the fovea uses almost wholly the 'green' process. While the fovea has long been suspected of blue-weakness, the negligibly small red value with day-adaptation is entirely unexpected. Ives used a substantially larger field, probably containing a larger area of parafoveal than of foveal retina. His work would be brought in line with this, if we can assume that in judging with the flicker photometer the true fovea is dominant, while with the method of equality of brightness the parafoveal region dominates in proportion to its larger area.

At the top of Fig. 11 I have plotted the curve of Pf_2 as estimated here, and the corresponding group of Sloan's observed values. It shows both the very good fit about the maximum and the divergence at the ends, indicating the need for some small activity of the other processes to explain the observations.

SUMMARY

The relative sensitivity of the eye to light of different wave-lengths varies with the brightness of the stimulus and especially with the state of adaptation of the eye (Purkinje phenomenon). If we assume that the sensitivity of the eye is primarily due to the presence of photosensitive pigments (visual purple and corresponding photopic substances) the curve of sensitivity of the eye should correspond to the sum of the curves of activity of these substances. It is assumed (*a*) that the band of activity of each substance is of simple form, to be represented by a probability curve (activity $= e^{-k(\lambda-c)^2}$;) (*b*) that the band for each substance is unchanged with state of adaptation of the eye, but that the relative proportion of the active pigments is changed;

It is found that the whole range of change of sensitivity of the eye with changing adaptation can be represented as the

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[11] The position of the absorption band of a colored substance depends on the substance in which it is dissolved. This amount of shift in wave-length seems large, however, to be explained merely on the basis of a difference of constitution between the foveal organs and ordinary cones.

conclusion with reference to the investigations just referred to, the particular problem of the relative efficiency of whole and part methods in the sphere of non-serial reactions is itself of importance; and this for two reasons: first, we have herein a field of learning in which the matter of relative efficiency is of consequence and to which the results obtained from work with serial reactions might not be wholly and unquestionably applicable; secondly, the relative efficiency in this field has some bearing on the explanation of the results of the experiments with serial reactions.

This latter statement may require elucidation. It may be said in general that the inferiority of a part method, when it appears, has been accounted for largely by the presumed failure therein to master the "act of connection," to use Pechstein's[7] phrase, between any part and the one next following. Indeed, not only the failure to master the proper connections, but even the actual establishment of wrong connections, as between the end of a part and its beginning, which must later be broken up and eliminated, has been supposed to occur. The conclusions reached in the texts, *e.g.* those of Woodworth,[8] Gates,[9] and Dashiell,[10] bear witness to the uniformity of opinion on this point. Again, the superiority of a progressive to a pure part-method, which has been demonstrated on every occasion when such a comparison has been made, has been attributed, as by Pechstein,[11] to the opportunity afforded in the progressive method for the practice of just those acts of connection and, of course, for the avoidance of fixation of the erroneous associations mentioned. The degree of interpretative emphasis actually laid upon these points is, if anything, understated in the comments above. For example, Pechstein writes: "It seems apparent that all the waste in part learning occurs in the act of connection."[12] And in another place he speaks of the "inherent advantages" of part methods (such as "the complete utilization of the transfer items"), and states that those advantages "together with the elimination of place associations, explain the universal superiority of modified part methods in motor learning."[13] Such conclusions unquestionably imply that part methods are in general more efficient, provided the difficulty consequent upon the "acts of connection" in acquiring serial reactions can be overcome.

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[7] L. A. Pechstein, Alleged elements of waste in learning a motor problem by the part method, *J. Educ. Psychol.*, 8, 1917, 303-310.

[8] R. S. Woodworth, *Psychology*, 1921, 345.

[9] A. I. Gates, *Elementary Psychology*, 1925, 366.

[10] J. F. Dashiell, *Fundamentals of Objective Psychology*, 1928, 359.

[11] Pechstein, *Psychol. Monog.*, *loc. cit.*

[12] Pechstein, *J. Educ. Psychol.*, 8, 1917, 303-310.

Now it would seem that if the inferiority of a pure part method to the whole method or to a progressive part method is to be explained on the bases stated, then—for the acquisition of reactions which are throughout non-serial, which lack any definite, fixed, or even predictable sequence, which are in a measure mutually isolated from one another and with reference to which, therefore, few if any definite acts of connection are to be acquired—some form of a part method, especially a pure part method, should either prove superior to any other or, at least, should be at a less than usual disadvantage. For it seems evident that in non-serial reactions any difficulty associated with acts of connection is at a minimum, and that a part method should therefore have every opportunity to display the "inherent advantages" which it has been alleged to possess. It is therefore to determine the relative economy of whole and part methods with non-serial reactions that we have undertaken this comparison of their efficiency for learning certain types of card sorting.

PROCEDURE

Subjects. The *S*s were 143 undergraduate men and women at Washington Square College, New York University. They were all from classes in psychology and mainly from those of the writer.

Materials. The materials used were as follows:

(1) A wooden box of 9 compartments, each compartment 4½ in. square, arranged in 3 rows of 3 each. The middle row was elevated above the front row, the back row above the middle row so that, while the height of the front of the box was 3⅞ in., that of the back was 11⅛ in. Each compartment used was labeled with some number from 1 to 10 inclusive (9 was omitted) by tacking to its rear side a flinch card bearing the appropriate digit. For the preliminary practice, however, the labels were the kings of the four playing-card suits.

(2) Packs of flinch cards, each containing 72 cards divided equally (when possible) among three to nine different denominations from 1 to 10 inclusive (9 omitted).

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[13] Pechstein, *Psychol. Monog.*, *loc. cit.*

verticals; upper, middle, lower horizontals, or the reverse, *i.e.* lower, middle, upper horizontals. Each kind of part (*i.e.* of row) and each order of practice was employed an equal number of times. The practice upon the parts was distributed as follows: trials 1 and 2, part 1 (*i.e.* some one row, vertical or horizontal); trials 3 and 4, part 2 (*i.e.* a second row); trials 5 and 6, part 3 (*i.e.* the third row). On trials 7, 8, 9 and 10 all three parts together (*i.e.* the whole arrangement complete) were employed. Hence *S* had 6 trials with the part method, and 4 with the whole method. Since a pack always contained 72 cards, each of the 6 trials with the single parts involved sorting 24 of each of the 3 different cards. The packs for the last 4 trials were identical with those used throughout in the whole method.

(*3*) *The combination part method.* The original parts were rows exactly as described for the pure part method, and both the kind of row and the order of practicing the rows were varied in precisely the same fashion. The practice was distributed as follows: trial 1, part 1; trial 2, part 2; trial 3, part 3. *S* had now had one trial with each of the three parts. He next proceeded to work with combinations of the parts already practiced individually, as follows: trial 4, parts 1 and 2 combined; trial 5, parts 1 and 3 combined; trial 6, parts 2 and 3 combined. Trials 7 to 10 inclusive were, as before, devoted to practice with the entire arrangement (*i.e.* to practice with the whole method). Since, as always, each trial involved sorting 72 cards, 24 of each of the three cards used were sorted in trials 1 to 3 inclusive, and 12 of each of the six cards used in trials 4 to 6 inclusive.

Comparing the three practice methods thus far described, it should be noted that when *S* commenced the 7th trial, which in all cases was with the entire arrangement, he had had, regardless of his previous method of learning, exactly the same amount of practice, namely, 48 sortings, with each individual card. For each card had been sorted as follows: in the whole method 8 times in each of 6 trials; in the pure part method 24 times in each of 2 trials; and in the combination part method 24 times in one trial plus 12 times in each of 2 trials. Hence, so far as these three methods are concerned, the *Ss* at the 7th trial did not differ at all in the amount of their previous practice with each individual card, but only in the kind of practice, whether with a whole, a pure part, or a combination part method.

(*4*) *The progressive part method.* In this method the part with which *S* began was, as before, a row of 3 compartments; and this row might be, also as before, the upper or lower horizontal, or the left or right vertical. He sorted one pack of 72 cards with this part. One compartment was then added to the original 3, making a total of 4 compartments, into which he sorted a pack containing 4 different cards. Another compartment was then added and he sorted a pack containing 5 different cards; and so on, adding at each trial one compartment to the arrangement and one card to the pack sorted, until on the 7th trial, again as before, the entire arrangement and all 9 cards were employed. This method of course rendered impossible an equal amount of practice with each card previous to the 7th trial. Instead, the practice on the individual cards was distributed as follows.

	No. of times sorted					
No. of trial	3 cards of original part	4th Card	5th Card	6th Card	7th Card	8th Card
1	24					
2	16	24				
3	12	12	24			
4	10	10	10	22		
5	8	8	8	8	24	
6	7	7	7	7	7	23
Totals	77	61	49	37	31	23

The selection of the compartments and of the cards for trials 2 to 6 inclusive was spatially determined. That is, if the original three-compartment part was the upper horizontal row, then the 4th compartment (the first one added) was the first compartment, reading from left to right, in the middle horizontal row; hence, in the arrangement reproduced on page 547, it was compartment 4. The 5th compartment (the second one added) was the next one in that row, namely, compartment 7. Then compartments 10, 8 and 3 were added, one at a time, in that order, until on the 7th trial compartment 6 was finally employed. If the original three-compartment part was the lower horizontal row, then the other compartments were added in an order exactly reversing that described above; namely, 10, 7, 4, 1, 5, 2. If the original part was the left vertical row, the compartments were added in the order 5, 7, 3, 1, 10, 6. And, finally, if the original part was the right vertical row, the order of addition was the reverse; namely, 3, 7, 5, 8, 4, 2. Each of the various original parts and orders of adding other compartments was used an equal number of times.

The complex card sorting. Two kinds of sorting were employed, one the 'simple', in which, as already described, each card was sorted into the compartment bearing the number identical with that on the card, and the other the 'complex,' in which the sorting was subjected to the complication introduced by, and according to, the paragraph in the instructions reproduced below. This paragraph was substituted for the first sentence in the second paragraph of the instructions for the simple card sorting already given.

"Cards bearing some particular number, as 4 for example, are not, however, to be sorted into the compartment bearing that same number. Instead the following procedure is to be adopted:

"All cards bearing the number 2 go into the compartment numbered 1, 3 into 2, 4 into 3, 5 into 4, 6 into 5, 7 into 6, 8 into 7, 10 into 8, and 1 into 10."

With the exception of this different method of sorting, the complex card sorting was identical with the simple in all the details of material and procedure heretofore described.

There was introduced, however, in the complex card sorting a modification of the combination part method which, in the presentation of results, will be termed *Combination Part Method B.* This form of the combination method differed from the one previously described in the composition of the various parts. In all the part methods thus far discussed each part consisted of a single row of the arrangement. Hence the choice of the individual cards that should constitute a part was spatially determined, and only by chance would the cards of each part bear to each other any but a spatial relation. In the was 461.3 sec., for the pure part method 267.6 sec., for the combination part 356.0 sec., and for the progressive part 364.3 sec. Differences of this kind permit such a question as: Is a method of learning which yields a speed of 63.3 sec. after 461.3 sec. of practice (which was the record of the whole method group) more economical than one which yields a speed of 74.2 sec. after 356.0 sec. of practice (which was the record of the combination part group)? The difficulty of answering such a question indicates that other criteria than speed on a single trial must be employed.

Let us therefore consider the relative efficiency of the four practice methods according to the second criterion mentioned above; namely, the amount of practice time required to attain various given speeds.

The selection of the 'given speeds' is necessarily somewhat arbitrary. We shall here consider the amount of practice time (and number of trials) required to attain the speeds of 75, 65 and 60 sec. only.

(a) To attain a sorting speed of about 75 sec. (Data from Table I.)

Group	Trial on which the above speed was approximated	Total of previous practice time
Whole	3	182.5
Pure Part	8	350.6
Combination Part	7	356.0
Progressive Part	7	364.3

Hence to attain a sorting speed of about 75 sec. the whole method was far superior to any part method. The three part methods were about equal in efficiency. But the pure part group attained the above speed only after one trial with the whole method, so that its equality with the other part method groups is somewhat doubtful.

(b) To attain a sorting speed of about 65 sec. (Data from Table I.)

Group	Trial on which the above speed was approximated	Total of previous practice time
Whole	6	397.4
Pure Part	9	418.2
Combination Part	9	497.8
Progressive Part	10	581.7

Hence to attain a sorting speed of about 65 sec. the whole method group was slightly superior to the pure part method group, and greatly superior to the combination and progressive part groups. Of the three part method groups the pure part was distinctly superior, the progressive part inferior. It should be noted, however, that no part method group attained such a speed as a result of practice with a part method alone, but only as a result of practice with both part and whole methods.

(c) To attain a sorting speed of about 60 sec. (Data from Table I.)

	Trial on which the above speed was approximated	Total of previous practice time
Whole	8	524.6
Pure Part	10	481.1
Combination Part	10	563.3
Progressive Part	Not attained	581.7

Hence to attain a sorting speed of about 60 sec. the pure part method was superior to the whole method group, the latter was superior to the other two part method groups, the progressive part group was inferior to all. But again it must be remembered that no part method group even approximated such a speed as a result of their practice with a part method alone. The pure part group, for example, required 4 trials with the whole method in addition to their 6 with the pure part method before they reached that speed.

The above results of the simple card sorting may be thus summarized:

(1) According to the criterion of sorting speed attained on a given trial, in particular the seventh, which may be considered to be the 'test trial,' amount but not time of practice being constant, the whole method was superior, the pure part method inferior, the combination and progressive part methods intermediate in efficiency.

(2) According to the criterion of the amount of practice time required to attain a given speed, the whole method was in general superior, especially for the attainment of the lower speeds. The combination and progressive part methods were throughout inferior to the whole method. The pure part plus whole method was superior to any part plus whole method for the attainment of the higher speeds.

Results: Complex Card Sorting

Table II gives, for all 5 practice methods, the average sorting speed and the cumulative totals of previous practice time (in sec.) for each of the 10 trials. In addition Table III below gives the number of errors made by each group on trials 7 to 10 inclusive.

As was the case with the simple card sorting, two criteria will be employed to determine the relative efficiency of the whole and of the four part methods; namely, the speed attained on a given trial and the amount of practice time required to attain a given speed.

As before, the sorting speed on the 7th trial must be considered as the test, the only unambiguous way of applying the first criterion. An inspection of Table II will show that the average speed on this trial was for the whole method group 85.6 sec., for the pure part method group 146.3 sec., for the combination part 108.2 sec., for the combination part B 106.2 sec., and for the progressive part 102.6 sec. Hence the whole method was without question

TABLE II

TRIAL	WHOLE METHOD		PURE PART METHOD		COMB. PART METHOD		COMB. PART METHOD B		PROG. PART METHOD	
	Speed	Total Time	Speed	Total Time	Speed	Total Time	Speed	Total Time	Speed	Total Time
1	146.4	0	65.2	0	63.4	0	67.7	0	70.1	0
2	119.2	146.4	49.7	65.2	65.9	63.4	72.9	67.7	68.7	70.1
3	104.1	265.6	66.8	114.9	58.6	129.3	80.2	140.6	76.7	138.8
4	98.9	369.7	49.9	181.7	102.7	187.9	106.0	220.8	89.8	215.5
5	89.8	468.6	59.1	231.6	98.6	290.6	112.2	326.8	95.4	305.3
6	91.7	558.4	46.0	290.7	95.7	389.2	95.1	439.0	99.7	400.7
7	85.6	650.1	146.3	336.7	108.2	484.9	106.2	534.1	102.6	500.4
8	82.7	735.7	111.9	483.0	97.0	593.1	93.6	640.3	92.9	603.0
9	80.1	818.4	101.7	594.9	86.4	690.1	87.1	733.9	91.6	695.9
10	76.2	898.5	93.9	696.6	83.7	776.5	84.0	821.0	85.3	787.5

The table should be read as follows: On the 5th trial the average of the whole-method group was 89.8 sec. The total practice time previous to the 5th trial was 468.6 sec. (*i.e.* the sum of the average speeds on the first 4 trials). The number of Ss in each group was 16, except in the pure part method group, where it was 15.

TABLE III

Total Number of Errors Made by Each Group

TRIAL	WHOLE METHOD	PURE PART METHOD	COMB. PART METHOD	COMB. PART METHOD B	PROG. PART METHOD
7	10	116	52	43	18
8	11	41	43	30	22
9	10	39	18	28	16
10	6	15	18	17	16

superior, the pure part method inferior, the other three part methods intermediate in value and about equal to each other, as preparation for work with the entire arrangement.[15]

Comparisons of the speeds attained on later trials can be made from Table II. The whole method group remains throughout superior, the pure part group inferior, the other part method groups intermediate and about equal to each other. Apparently the practice from the 7th trial on with the entire arrangement, which all the part method groups of course had, did not enable them even to approximate the speed of the whole method group.

As we saw in the case of the simple card sorting, however, the amount of time spent in practice previous to any given trial was very unequal for the several groups. Hence the second criterion, that of the amount of practice time required to attain various sorting speeds, should be applied. The speeds chosen for such comparison are 105 and 85 sec.

(a) To attain a sorting speed of about 105 sec. (Data from Table II.)

Group	Trial on which the above speed was approximated	Total of previous practice time
Whole	3	265.6
Pure Part	9	594.9
Combination Part	7	484.9
Combination Part B	7	534.1
Progressive Part	7	500.4

Hence to attain a sorting speed of about 105 sec. the whole method was by far the superior, the pure part method inferior, the other part methods intermediate in value and about equal to each other. With respect to the pure part method groups, it may be further noted that they attained the above speed only after a practice with the entire arrangement amounting to 2 trials and 258.2 sec.

(b) To attain a sorting speed of about 85 sec. (Data from Table II.)

Group	Trial on which the above speed was approximated	Total of previous practice time
Whole	7	650.1
Pure Part	Never attained.	
Combination Part	9	690.1
Combination Part B	9	733.9
Progressive Part	10	787.5

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[15] As to the reliability of these differences:

	Difference	S.D.
Whole—Pure Part	60.7	10.49
Whole—Combination Part	22.6	4.85
Whole—Combination Part B	20.6	7.50
Whole—Progressive Part	17.0	5.92
Pure Part—Combination	38.1	10.68
Pure Part—Combination B	40.1	11.69
Pure Part—Progressive	43.7	10.75
Combination—Combination B	2.0	7.76
Combination—Progressive	5.6	6.24
Combination B—Progressive	3.6	7.86

Hence to attain a sorting speed of about 85 sec. the whole method group was superior, the pure part group inferior. The other part method groups were intermediate in attainment; their efficiency relative to each other is obvious from the table.

The error scores shown in Table III should also be noted. It will be seen that the number of errors made by the part method groups when working with the entire arrangement greatly exceeded those made by the whole method group; and further, that the errors made on the 7th trial by the pure part group greatly exceeded those made by any other group on that trial. In fact they even exceeded the number (which was 102) made by the whole method group on their first trial.

The results of the complex card sorting may be thus summarized. According to the criteria both of the sorting speed attained on a given trial, especially the 7th, and of the amount of practice time required to attain given sorting speeds, the whole method was superior, the pure part method inferior. The other three part methods were intermediate in efficiency, and were about equal to each other. These differences were much greater and more consistent than they were with the simple card sorting.

Discussion of the Results

The results of the experiments with simple and complex card sorting may be stated as follows.

(1) The whole method was in general superior to any part method for learning the non-serial reactions involved.

(2) Of the several part methods employed the pure part, in which there was no practice with parts in combination previous to work with the entire arrangement, was in general the least advantageous.

(3) The various forms of part methods in which such practice of parts in combination or progression occurred were intermediate in efficiency between the whole and the pure part method. Since such part methods resemble a whole method more than a pure part method does, their superiority to the latter serves if anything to lend support to the first two conclusions above.

(4) All of the conclusions above apply even more clearly and unquestionably to the complex than to the simple card sorting.

The general applicability of these conclusions to the learning of all non-serial reactions is of course doubtful. The present experiments dealt with one type of non-serial reaction only, that involved in card sorting. Further, the card sorting was on the whole of a rather simple kind; only a small number of different cards and compartments was used, the total amount of practice was very brief, and in the part methods the parts and the number of trials allotted to each part were arbitrarily chosen. It is possible that greater complexity of reaction, a larger number of cards, a different amount of total practice, a different selection of or a different amount of practice on the parts might have favored, even in the field of card sorting itself, some form of a part method. Nevertheless the results of the experiments appear, with due regard to the limitations stated, to be valid and to justify some attempt at explanation.

There are, we believe, two aspects of the results which in particular require accounting for; (1) the inferiority of any part method to the whole method; and (2) the greater inferiority of part methods in the complex than in the simple card sorting.

With reference to the question as to why the part methods were inferior to the whole method, the following explanations among others are theoretically possible.

(*a*) *Negative transfer.* It is possible that negative transfer from one part to a succeeding one occurred. Inspection of Tables I and II, however, shows clearly that transfer is, if anything, positive in most cases. For example, the sorting speed of the pure part method group is greater on trial 3, its first trial with part 2, than it is on trial 1, its first trial with part 1; and likewise is greater on trial 5, its first trial with part 3, than it is on trial 3. Pechstein, it may be noted, also found only positive transfer from one section of his maze to another.[16]

(*b*) *Retroactive inhibition.* It is possible that practice on one part might impair retention of parts previously practiced. We have no data on this point; but Pechstein found no evidence of such inhibition in learning mazes by the part method.[17]

(*c*) *Forgetting.* The lapse of time between the completion and the resumption of practice with a part might impair retention thereof. This possibility is most evident in the pure part method where, for example, *S* after his initial trials with Part 1 did not encounter the cards and compartments of that part again until he reached the entire arrangement on the 7th trial. It seems improbable, however, that retention was significantly affected from this cause; for the interval of time between such completion and resumption of practice was in all cases very short, and in the progressive part method it was merely the constant interval between all trials by any method, namely, one minute.

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[16] Pechstein, *loc. cit.*

[17] Pechstein, *ibid.*

(*d*) *The 'ease' of the parts.* In both the simple and the complex card sorting the mastery of the parts seemed very easy. By the second trial with each part (in the pure part method), or even before the end of the first, the sorting had become largely automatized and proceeded with great speed and few if any errors. The work on the parts may therefore have been so easy that it failed to stimulate a degree of incentive, effort, concentration adequate for maximum efficiency when work with the entire arrangement was begun. Or it may have occasioned in the *S*s a false notion as to the difficulty of their task as a whole, a premature over-confidence and over-expectation, which was a definite hindrance when they came to the much harder work of sorting with the entire arrangement. Such factors may have had some deleterious influence; but we have no data to show whether or not they did have.

(*e*) *Massed vs. distributed practice.* In all this card sorting the practice may be classified as having been massed rather than distributed, since the interval between trials was always one minute only and the entire series of trials was in most cases completed within a half-hour. In his monograph on maze learning, Pechstein points out that the whole method was at a special disadvantage when practice was massed.[18] He further remarks that "he is inclined to argue from his results to the general motor field," within the limits of which card sorting surely lies, and likewise that "under massed conditions the part methods are always more efficient." Had part methods been so in the present experiment, we might have utilized the above conclusions as a partial explanation thereof. But since our results run counter to Pechstein's generalizations, we would suggest rather that the latter need some sort of qualification or amendment.

In a later article, however, Pechstein relates the double problem of part *vs.* whole methods on the one hand and massed *vs.*

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[18] Pechstein, *ibid.*

distributed practice on the other to a third factor, namely, the difficulty of the problem, and in this connection writes: "The longer and more difficult the problem, the more advisable to break it up into units and learn both the units and the connection of these under massed conditions . . . The hard problem becomes easy if it is learned under massed conditions by the part method and in no other way; it remains hard if it is learned as a whole under massed or distributed conditions, or even in parts under distributed conditions."[19] It might be inferred from the above that with easy problems Pechstein would expect the combination of massed practice and whole method to be less disadvantageous. Since both the simple and the complex card sorting employed in our experiments are, we presume, to be classified as easy problems, our finding the whole method superior to any part method even when practice was massed is perhaps less contrary to his conclusions than might at first appear. Nevertheless, if we take his generalizations literally, we would at least expect to find the whole method of less advantage in the complex card sorting, which was the harder problem learned under massed conditions, than it was in the simple, which was much the easier problem, provided, that is, that other things were equal. One can not of course assume that other things were equal; but one may perhaps suggest that the whole method can not be so decisively inferior for the learning of a harder problem under massed conditions as Pechstein's conclusions would imply.

(*f*) *Relationships among the reactions practiced.* We have already pointed out that failure to connect the parts properly is the particular disadvantage from which a pure part method is supposed to suffer and which some sort of progressive or combination part method is supposed, usually if not always, to eliminate. We have further indicated that in non-serial reactions this disadvantage should be at a minimum and that the learning of such reactions should therefore provide a field especially favorable to a part method. In card sorting the reactions are certainly not serial; they follow no invariable sequence; they do not constitute *in toto* an integrated closely ordinated whole. Nevertheless it would be going too far to claim that these non-serial reactions were wholly isolated from, and independent of, each other. (1) For one thing, an important phase of the work was learning to find rapidly the particular compartment into which any given card was to be sorted, a process which required selection and fixation of the appropriate eye- and the coördinated hand-movements. It is quite conceivable that this process was assisted by observation of the numerical and spatial relationships between differently numbered compartments, *e.g.* by seeing that the three compartments on the left were 2, 4 and 8, all of which are even numbers; or that 2 and 1 occupy the two upper corners but in a left-right sequence reversing their order in the number series; or that 7 was to the right of four, 10 just below 1, 4 just above 8. Such observations were made by the *S*s; they may have been of importance to rapid learning; they may have been made to better advantage when there was throughout a visual presentation of all, or a greater part of, the nine numbers on the nine compartments. Hence the whole method may have favored their acquisition and the part methods, especially the pure part part method, been unfavorable thereto. (2) Another factor of possible significance was the development of more rapid and accurate discrimination between one card and another as they appeared in the pack; *e.g.* between 3 and 8, which some *S*s found confusingly similar. Such discrimination might be facilitated by the use of packs containing all nine cards, that is, by those used throughout in the whole method. There is no evidence to suggest, however, that this factor was of any great significance. With respect, then, to this matter of relations, we are of the belief (1) that although the reactions practiced were non-serial, they were in actual fact brought, in some measure, into relation with each other by the *S*s; certainly it seems improbable that any group of responses practiced together ever could remain wholly discrete one from the other; and (2) that the establishment of such relationships may have been important to learning and have been favored by practice with the whole method.

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[19] Pechstein, *J. Educ. Psychol.*, 12, 1921, 92-97.

The final conclusion with reference to the first problem stated earlier, namely, the reason for the rather consistent inferiority of part methods and especially of the pure part method, to the whole method must unfortunately be that no completely satisfactory explanation is at hand, unless indeed the above factor of establishing relations be considered adequate. The writer expected to find part methods greatly superior to whole methods in learning the non-serial reactions of card sorting. This expectation was not realized, and for the contrary results we can not fully account at present. But the fact that the results did not accord with a legitimate expectation does, we believe, justify these two conconclusions. (1) That since the above described relations were not serial connections, one can not state as a universally applicable law that *all* the waste in part learning occurs in the act of such connection; and (2) that since no form of combination or progressive method proved equal in efficiency to the whole method, one likewise can not assert as a universally applicable law that such modifications of the part method, in which the above-mentioned waste would presumably be avoided, must be expected *always* to be superior to the whole method.

The second problem cited earlier for explanation was the greater superiority of the whole method and the greater inferiority of the pure part method in the complex card sorting. The principal differences between the simple and the complex sorting seem to be the greater difficulty of the latter, and the potentially greater number and complexity therein of the relations between the various reactions to be learned.

With reference to the first difference above there is little additional to be said. A part method might be expected to be more advantageous in the harder of two problems learned by a massed method, but it was as a matter of fact less so. With reference to the second difference, however, some analysis is possible. We have already noted that in the simple card sorting the various reactions, though certainly not serial, were nevertheless not wholly independent of each other. In the complex sorting they were probably even less discrete. For in this type of sorting not only do all the relationships mentioned as present in the simple card sorting exist, but also, as was pointed out on page 550, an important phase of the learning was the acquisition of certain inhibitory reactions which were not demanded in the simple card sorting. Among these was, for example, learning to inhibit the reaction of sorting card 6 into compartment 6, a response more "natural" than that of sorting 6 into compartment 5, which was the act required; or inhibiting the reaction of putting card 6 into compartment 7, which is the compartment numerically one above instead of one below the card to be sorted. Since it is generally agreed that a response, whether classified as inhibitory or not, can not be acquired unless it is in some sense made, the most satisfactory development of these inhibitions would require that the reaction to be inhibited be itself made or in some manner initiated; and this in turn would require that its stimulus be present. Therefore to provide the maximum possibility of learning all the inhibitions potentially involved, it would seem advantageous to have had all the compartments presented with their proper numbers and so open to visual observation and to the chance of erroneous response to them. This condition, however, was met completely only in the whole method and was satisfied least adequately in the pure part method. Certainly some factor was present in the complex sorting which decisively favored the whole method. And if that factor was this greater complexity of relationships, then the differences in the results of the simple and the complex card sorting are perhaps explained. But since these relationships were not serial connections, and since even in the complex sorting neither combination nor progressive part methods proved equal in efficiency to the whole method, the analysis here merely lends additional weight to our previous criticisms of existing generalizations concerning the sole source of waste in part methods and the uniform desirability of a combination or a progressive rather than a whole method.

SUMMARY

The purpose of the experiment was to compare the efficiency of the whole and of three types of part method, a 'pure,' a 'combination,' and a 'progressive,' for learning certain non-serial reactions, namely, those involved in two types of card sorting, one simple, the other relatively complex. The criteria of efficiency were (a) the sorting speed attained on some given trial with the 'whole,' *i.e.* with all the cards and compartments used; and (b) the amount of practice time required to attain some given speed therewith. According to both criteria the whole method was in general superior, the pure part method inferior, the combination and progressive methods intermediate in efficiency and about equal to each other. The above differences were greater and more consistent for the complex than for the simple card sorting. Since experiment and theory in the field of serial reactions have suggested that the inferiority of part methods, especially of a pure part method, when found is due to the failure therein to give practice to the act of properly connecting certain responses with succeeding ones, it would be expected that in non-serial reactions, where there are no such connections to be acquired, part methods, and especially a pure part method, would possess an advantage far greater than common. The fact that all the part methods, even those of combination and progressive types, were definitely inferior for learning card sorting under the experimental conditions employed indicates: (1) that the crucial reason for the inferiority of a part method need not in all cases reside in this matter of serial connections (even though it may in part be found in failure to master relations other than serial); (2) that a combination or progressive part method is not necessarily superior to a whole method, even in the field of motor learning; and (3) that in consequence existing generalizations to such effect stand in need of qualification.

THE RELATIVE RÔLES OF THE TEMPORAL AND THE INTENSIVE FACTORS IN SOUND LOCALIZATION[1]

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I. INTRODUCTION

The outstanding problem, as indicated by current theories,[2] in the field of auditory localization is the determination of the relative rôles of the different factors that condition directional localization. Some investigators hold that localization is a function of the binaural integration of a single factor, either phase, time, or intensity, and we have the corresponding theories, viz., the phase-theory, the time-theory, and the intensity-theory. Others hold that phase and time are reducible to intensity, which gives us a further interpretation of the intensity-theory. Still others maintain that all of the factors are effective as such, but they would limit the effectiveness of each factor to a certain frequency range, phase being effective in the low range, intensity in the very high, while all three of the factors are more or less equally effective in the intermediate frequency range. This may be called a phase-time-intensity-theory. The writer believes that these three factors, and possibly also mass, contribute in a varying degree, depending upon the conditions, to a binaural difference pattern that results in directional localization. This may be called a 'difference-pattern-theory.' All of these theories and interpretations are supported by experimentation; but it seems that so much theorizing serves only to define the problem of the specificity of the stimuli more clearly, and to indicate that this problem requires much more, and perhaps more systematic, experimentation.

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[1] From the Psychological Laboratory, State University of Iowa.

[2] The following review the theory of sound localization: C. E. Ferree and R. Collins, An experimental demonstration of the binaural ratio as a factor in auditory localization, this JOURNAL, 22, 1911, 250-297; H. M. Halverson, Binaural localization of tones as dependent upon differences of phase and intensity, *ibid.*, 33, 1922, 178-212; E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, *ibid.*, 37, 1926, 157-188; C. A. Ruckmick, Recent acoustic research, *Psychol. Bull.*, 21, 1924, 617-621; also, Auditory sensations and related phenomena, *ibid.*, 24, 1927, 81-99; also, Facts and theories of audition, *ibid.*, 25, 1928, 229-244; O. C. Trimble, The theory of sound localization: a restatement, *Psychol. Rev.*, 35, 1928, 515-523.

The present investigation has approached the problem in a series of experiments dealing with the temporal and the intensive factors. We wished to determine which factor, if either, is the more effective in conditioning directional perception.

Previous investigations have dealt more or less directly with this particular problem. Klemm found that an intensive difference of a 3 to 1 ratio could be equalized by a temporal difference;[3] *i.e.* the phantom sound localized toward the ear receiving the more intense stimulus could be brought to the median plane by gradually increasing the temporal disjunction so that the ear receiving the weaker stimulus was the ear first stimulated. He reports, however, that the phantom remained on the side of the stronger stimulus during the smaller temporal differences, and that it tended to flash over to the earlier stimulated ear during the larger temporal differences. He reports also that localization was more indefinite and uncertain under these conditions.

Von Hornbostel and Wertheimer report that when the phantom sound was localized toward the side of the ear receiving the prior stimulus, if the prior stimulus was gradually weakened, the angular displacement of the phantom sound remained unchanged until the prior stimulus-sound was completely blotted out, at which time the phantom jumped suddenly over to the other side.[4]

Wittmann apposed intensive differences with a time-difference, and temporal differences with an intensity-difference, and found that both factors could supplement or neutralize each other to a varying degree in their effect upon localization.[5]

Banister[6] apposed phase differences[7] with intensive differences and found that a difference of phase at the ears might cause a displacement of the phantom sound even when the intensive differences were relatively very large. In another study this investigator apposed intensive differences with a phase difference of 90° and found that it took a binaural intensive ratio of at least 2.5:1 to produce the same angular displacement of a tone of 220-300 d.v. as did the phase difference.[8]

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[3] O. Klemm, Über den Einfluss des binauralen Zeitunterschiedes auf die Lokalisation, *Arch. f. d. ges. Psychol.*, 40, 1920, 117-146.

[4] E. M. von Hornbostel und M. Wertheimer, Über die Wahrnehmung der Schallrichtung, *Sitzungsb. d. preuss. Akad. d. Wissensch.*, 1920, 388-396.

[5] J. Wittmann, Beiträge zur Analyse des Hörens bei dichotischer Reizaufnahme, *Arch. f. d. ges. Psychol.*, 51, 1925, 21-122.

[6] H. Banister, A preliminary note on a new method of determining the phase effect in the localization of sound, *Brit. J. Psychol.*, 13, 1923, 435-436.

[7] Phase-difference may be considered as a special case of time-difference. Cf. G. W. Stewart, The intensity logarithmic law and the difference of phase effect in binaural audition, *Psychol. Monog.*, 31(1), 1922, 41; E. G. Boring, *op. cit.*, 166 ff.; H. M. Halverson, *op. cit.*, 200; O. C. Trimble, The phase-theory as a time-theory in sound localization, *Proc. Iowa Acad. Sci.*, 24, 1922, 286.

[8] H. Banister, Three experiments on the localization of tones, *Brit. J. Psychol.*, 16, 1926, 265-292.

Halverson found that large differences in the intensive ratio were required to effect even small changes in localization conditioned by phase differences; and that with intensive differences localization varied only slightly, except with very large differences in intensity, under which conditions double lateral localization resulted.[9] He concluded that localization with intensive differences may be discontinuous and not uniform as is the case with phase differences.

This review of previous studies shows that the findings are more or less contradictory; but they indicate that time may be the more effective factor in determining localization.

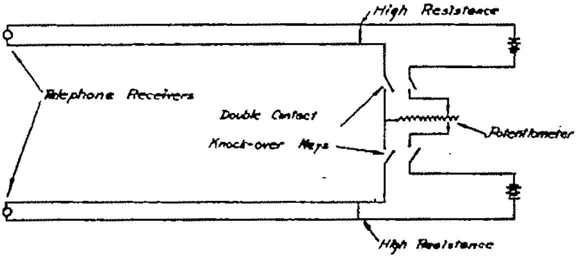

FIG. 1. DIAGRAM OF THE ELECTRIC CIRCUITS

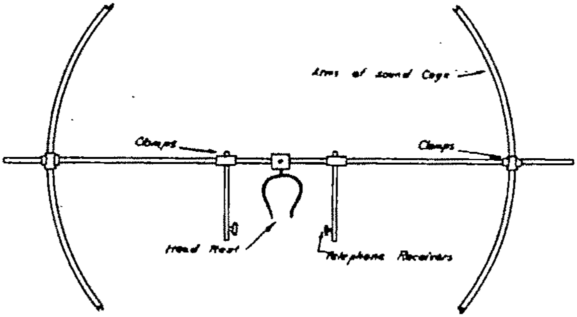

FIG. 2. DIAGRAM OF THE RECEIVER SYSTEM

II. PROBLEMS, PROCEDURES, AND RESULTS

A discrete impulse technique was employed in the present investigation.[10] The stimulus-sounds were generated by breaking electric currents (see Fig. 1) through telephone receivers of identical type. The receivers were fixed in a system (see Fig. 2) attached to the swinging arms of a Seashore sound cage,[11] so that they were kept at a constant distance, 6 cm. from the ears. The receiver membranes were adjusted so that they generated high-pitched sounds, because localization with low-pitched sounds proved difficult. The open air method of delivering the stimuli was employed, which made it necessary to use very weak sounds in order to avoid the second phantom sound[12] which is caused by the diffraction of sound about the head.

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[9] Halverson, *op. cit.*, 203 ff.

[10] The technique was a modification of the discrete impulse technique which has been described in detail in a previous article. O. C. Trimble, A discrete impulse technique in sound localization, *Brit. J. Psychol.*, 19, 1928, 167-178.

[11] C. E. Seashore, A sound perimeter, *Psychol. Rev.*, 10, 1903, 64-68.

The temporal factor was controlled by means of a pair of double knock-over contact keys, the relative positions of which could be varied by changing the set-screws. The series of time-differences were presented in terms of sigma. A Dunlap pendulum[13] was used to throw the contact keys, thus breaking the circuits. By means of a potentiometer with which the resistance in one circuit could be decreased while the resistance in the other circuit was increased, the intensive factor was controlled. The intensive series was presented in terms of δ, the ratio of the strength of the currents (milliamperes) passing through the receivers at the different points.[14]

Observers. The *Os*, all graduate students in psychology, had served in a previous investigation of considerable length, in which a discrete impulse technique, much the same as that used in the present study, was employed.[15] The *Os* were seated, singly, in a sound-proof room which was darkened a few seconds before and during each presentation of the stimuli. They were instructed to record the direction and the angular displacement of the sounds. Ascending and descending series were presented on both sides, and in such a way that the *Os* could not follow any preconceived plan.

Problems. With this technique, the problem was attacked from five different angles: (*1*) localization as a function of intensive differences; (*2*) localization as a function of temporal differences; (*3*) localization with intensive differences apposed with a temporal difference; (*4*) localization with temporal differences apposed with an intensive difference; and (*5*) localization with both factors varied together in the same direction.

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[12] This phenomeon was discovered in a previous investigation. O. C. Trimble, Some temporal aspects of sound localization, *Psychol. Monog.*, 38, 1928, (no. 1), 172-225.

[13] K. Dunlap, A new laboratory pendulum, *Psychol. Rev.*, 19, 1912, 240-245.

[14] A ratio of 6.57, for example, would mean an intensive ratio of 43.16 at this particular point in the series, if the laws of physics were applicable: viz., (1) that the amplitude of the vibration of the receiver diagram is directly proportional to the strength of the current passing through the receiver, and (2) that the acoustic energy in the sound wave is directly proportional to the square of the amplitude. The writer believes that these laws do not absolutely hold in this case because (1) the receiver membranes were weighted on both sides with heavy cardboard washers (thus limiting the vibrating surfaces so that they generated high-pitched sounds), which doubtless interfered with the force of induction. Also, (2) since receiver membranes are practically 'deadbeat,' a single swing of the membrane may not have been translated into acoustic energy to the degree that a continuous, uniform vibration is. These conditions were so complex that it was decided best to let the intensive series stand in terms of the ratio of the strength of the currents; but no doubt the binaural intensive ratio in the last part of the series was very much greater than that which results from the head-shadow in normal conditions of hearing.

[15] O. C. Trimble, *op. cit.*, *Psychol. Monog.*, 183 ff.

(*1*) *Localization as a function of intensive differences.* The first experiment attempted to determine whether localization on the basis of the intensive factor is discontinuous as has been suggested by Halverson,[16] or whether it is continuous and uniform as was found by Stewart and Hovda.[17] These investigators employed continuous sounds, namely tones, as stimuli, but they controlled the intensive factor by different means. Halverson used a rheostat with which the resistance in one receiver circuit could be increased while the resistance in the other circuit was decreased. Stewart and Hovda controlled the intensity of the stimuli by varying the distance of the receiving tubes from the sound source. This method introduced a phase difference which the investigators held was so slight as to be negligible, but it may have been sufficient to bridge the gap which Halverson found.

Results. Table I presents the data, from which it is evident that angular displacement is continuous and more or less uniform. The estimates for all of the *O*s, except *M*, gradually increase as the intensive ratio increases, until maximal lateral localization is reached. For *M* the threshold of localization is not reached until the binaural ratio is relatively large. After this threshold is reached the phantom changes rapidly to the maximal lateral position. In order to determine whether *M*'s was a case of somewhat discontinuous localization, a series of much finer intensive differences ranging above his localization threshold was offered, and his estimates showed a gradual and more or less regular increase. The average deviations show that localization is more or less consistent for each of the *O*s. Localization near the medial and the maximal lateral positions seems to be more consistent than it is at intermediate points.

The *O*s described the course of the phantom sound as corresponding somewhat to a semi-circle, a 90° arc on either side of the median plane.

It appears, therefore, that localization on the basis of intensive differences, when discrete sounds are used as stimuli,[18] is continuous and more or less uniform.

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[16] H. M. Halverson, *op. cit.*, 210.

[17] G. W. Stewart and O. Hovda, The intensity factor in binaural sound localization: an extension of Weber's law, *Psychol. Rev.*, 25, 1918, 242-251.

TABLE I

LOCALIZATION AS A FUNCTION OF INTENSIVE DIFFERENCES

Intensive ratio	Observers			
δ	*H*	*Hd*	*L*	*M*
1.00	X*	X	X	X
1.13	1	4	1	X
1.27	3	10	4	1
1.42	5± 3	14±11	7± 4	1± 2
1.60	7± 3	19±17	11± 4	2± 3
1.80	12± 4	29±18	15± 4	3± 4
2.03	15± 7	38±18	21± 4	2± 3
2.28	21± 8	47±21	24± 4	3± 4
2.57	27± 8	54±17	28± 4	5± 4
2.87	35±11	62±20	34± 5	7± 6
3.25	45±14	68±17	39± 6	14±12
4.11	59±15	73±14	47± 5	20±19
5.20	73±10	77±10	56± 6	34±21
6.57	86± 4	82± 6	66± 5	48±25
8.32	88± 2	85± 1	74± 5	73±17
10.53	90± 0	87± 2	81± 4	79±11
11.84	90± 0	89± 1	85± 1	88± 4

*The numbers given in this table are averages, with average deviations, of approximately 10 estimates, by the individual *Os*, of the angular displacement of the phantom sound from the median plane, at the different points in the intensive series. X represents an unknown because of slight errors in localization. When no average deviation is given, the number represents an estimated average, because of errors in localization.

(*2*) *Localization as a function of temporal differences.* Localization on the basis of the temporal factor has been described in a previous article;[19] but in order to establish comparable units of angular displacement, and to make direct comparison possible, localization with time-differences was determined under the conditions that obtained in the foregoing experiment.

Results. Table II summarizes the results. A comparison of this table with Table I shows that angular displacement with intensive differences increases more rapidly on the basis of the intensive increment employed than does angular displacement with time-differences on the basis of the temporal increment employed. Otherwise the localization in both cases is essentially the same, except for the further fact that in localization on the basis of the temporal factor double lateral localization was reported before the end of the series, at approximately 2σ.

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[18] Continuous sounds are so complex, due to diffraction of sound about the head, and to auditory persistence, summation, and fusion, that the present writer doubts if any of the factors under consideration can be adequately controlled under such conditions.

[19] O. C. Trimble, *op. cit.*, *Psychol. Monog.*, 220 ff.

TABLE II

Localization as a Function of Temporal Differences

Temporal differences	Observers			
σ	*H*	*Hd*	*L*	*M*
000	x	x	x	x
.12	4	9	6	6
.24	7± 1	17± 6	9± 1	10± 5
.36	11± 7	28± 6	13± 2	15± 8
.48	17± 5	31± 5	13± 2	22± 7
.60	21±10	35± 8	15± 2	30± 9
.72	29±10	29± 7	17± 3	35±14
.84	37±11	42± 6	17± 2	45±16
.96	44±12	44± 7	20± 4	58±15
1.08	49±12	50±10	21± 3	64±19
1.20	57±14	50±10	23± 6	65±24
1.32	66±15	54±11	37± 8	72±25
1.44	72±16	54±11	39± 5	72±21
1.56	72±18	58±14	39± 5	70±16
1.68	74±14	52± 6	40± 4	72±17
1.80	79±11	53± 7	41± 4	76±17
1.92	82± 8	58± 9	43± 6	72±14
2.04	86± 3	61±11	49±15	74±19
2.16	87± 3	63± 9	53±14	70±20
2.28	88± 1	65± 8	54±18	64±19
2.40	88± 1	66± 9	55±10	68±22
2.52	90± 0	65± 9	58±10	68±22
2.64	90± 0	65± 9	63± 9	76±17
2.76	90± 0	65± 9	67± 8	80±12
2.88	90± 0	70±10	73± 7	82± 9
3.00	90± 0	70±10	78± 5	86± 6
3.12	90± 0	75±10	81± 5	88± 3
3.24	90± 0	76±11	84± 3	90± 0

The temporal and the intensive series may be considered as fairly well balanced, since the fused sound divides at about the same time in the temporal series that the phantom sound comes to the maximal lateral position in the intensive series.

These series, or units of these series, were employed in the remaining experiments of this analysis.

(3) *Localization with intensive differences apposed with a temporal difference.* The third experiment of the series dealt with the effectiveness of intensive differences apposed with a relatively large temporal difference. The purpose was to determine the effect on a localization conditioned by a time-difference of presenting the stimuli so that the later stimulus was also the stronger stimulus. The stimuli were presented with a temporal disjunction of 1.92σ in order to obtain as nearly as possible a maximal lateral localization without the double lateral effect. It was thought best to avoid the double lateral effect because under such condi-

TABLE III
Localization with Intensive Differences Apposed With a Relatively Large Time-Difference

Intensive ratio	Observers							
	H		*Hd*		*L*		*M*	
δ	I	T	I	T	I	T	I	T
1.13	90± 0	85±10	—	74±13	—	63± 9	—	69±21
1.27	90± 0	89± 1	—	61± 9	—	59±13	—	68±27
1.42	90± 0	90± 0	—	49±13	—	36± 9	—	58±25
1.60	90± 0	90± 0	—	43±16	—	42±17	90± 0	56±15
1.80	90± 0	90± 0	—	37±21	17± 2	35±14	90± 0	41± 4
2.03	83± 3	90± 0	—	25±17	17± 2	18± 5	84± 9	37± 9
2.28	89± 1	83±11	?	16±17	20± 0	18± 3	85± 7	52±18
2.57	77±18	75±15	?	17±17	22± 2	17± 5	79±16	31± 9
2.87	90± 0	78±18	35±16	?	20± 3	18± 3	90± 0	28± 9
3.25	90± 0	79±18	47±22	?	25± 5	13± 5	72±21	26±12
4.11	90± 0	66±23	?	?	26± 4	15± 2	78±19	30± 4
5.20	90± 0	58±32	47± 2	—	26± 4	15± 4	85± 9	36±22
6.57	90± 0	62±28	70±13	—	32±13	15± 4	90± 0	46±19
8.32	90± 0	?	87± 4	—	33±14	?	81±14	?
10.53	90± 0	63± 9	87± 4	—	58±25	—	90± 0	—
11.84	90± 0	—	88± 3	—	76±14	—	90± 0	—
12.32	90± 0	?	89± 1	—	85± 5	—	90± 0	—

These numbers are averages, with average deviations, of 5 estimates, by the individual observers, of the angular displacement at the different points in the intensive series.

— indicates that sound was not reported.

? indicates that the *Os* were uncertain whether they heard the sound or not, or that they occasionally reported double lateral localization.

tions the influence of the intensive factor might not be evident as soon as it otherwise would, since there would be no way of knowing to which factor the 'effect' was due.

Results. A summary of the data is presented in Table III. This table shows that two sounds, one on the side of the prior stimulus and also one on the side of the stronger stimulus, are in general localized simultaneously in the middle range of the series, with a general tendency for the sound on the side of the stronger stimulus to appear at about the third point from the beginning of the series, and for the sound on the side of the prior stimulus to disappear at about the third point from the end of the series.

The first slight intensive difference is sufficient to introduce a double lateral effect in *H*'s case, and the sound on the side of the stronger stimulus remains localized very consistently at the 90° position throughout the series, while the sound on the side of the prior stimulus falls back somewhat toward the median plane and is finally blotted out near the end of the series when the intensive difference is relatively very large. Double lateral localization on the basis of the temporal factor may have been present in this case at the beginning of the series; but in *M*'s case, which is much the same as that of *H*, the double lateral effect does not appear until the fourth step in the series is reached, which means that this effect was not due to the temporal factor in this case, and also that it may not have been due to the temporal factor in *H*'s case.

In the case of *Hd*, and also *L*, the sound localized toward the side of the prior stimulus falls more or less gradually from the beginning of the series toward the median plane to a localization of approximately 15° and finally drops out altogether, while the sound on the side of the stronger stimulus does not appear until rather late in the series, at which time it is localized near the median plane, but changes fairly gradually to the maximal lateral position as the end of the series is approached.

The average deviations indicate that the localization toward the side of the prior stimulus is more difficult than the localization toward the side of the stronger stimulus.

The results therefore indicate that intensive differences not only neutralize the 'effect' of the temporal factor, but also that such differences may either introduce a double lateral effect or condition approximately the normal 'intensity effect.'

(*4*) *Localization with temporal differences apposed with an intensive difference.* The next step in this analysis was the determination of the effectiveness of temporal differences apposed with a relatively large intensive difference.– The conditions of the previous experiment were reversed, *i.e.* the stimuli were presented so that the weaker stimulus was also the prior stimulus. The stimuli were presented with an intensive difference sufficiently large to cause approximately a maximal lateral localization. In terms of the units of the intensive series, the binaural ratio was 8.32δ.

Results. The results are summarized in Table IV. A comparison of this table with Table III reveals the fact that only one sound appears when temporal differences are apposed with a relatively large intensive difference, and that this sound is always localized on the side of the stronger stimulus and fairly consistently at the maximal lateral position.

H localizes the sound at 90° position at every point in the series. *Hd* tends to localize the sound somewhat toward the median plane as the temporal difference increases, but the average

TABLE IV

Localization with Temporal Differences Apposed with a Relatively Large Intensity-Difference

Temporal difference	Observers							
	H		*Hd*		*L*		*M*	
σ	I	T	I	T	I	T	I	T
0.12	90±0	—	83± 3	—	87±2	—	75±22	—
0.24	90±0	—	89± 1	—	88±2	—	80±10	—
0.36	90±0	—	87± 3	—	85±6	—	80±10	—
0.48	90±0	—	81± 5	—	81±4	—	80±15	—
0.60	90±0	—	82± 6	—	83±2	—	82±11	—
0.72	90±0	—	86± 5	—	82±3	—	80±10	—
0.84	90±0	—	83± 3	—	80±4	—	70±13	—
0.96	90±0	—	82± 3	—	80±2	?	70±17	—
1.08	90±0	—	83± 4	—	79±2	?	70±22	—
1.20	90±0	—	82± 5	—	80±0	?	70±22	—
1.32	90±0	—	77±11	—	79±4	?	76±16	—
1.44	90±0	—	77±11	—	82±2	—	82± 8	—
1.56	90±0	—	76±13	—	83±2	—	86±10	—
1.68	90±0	—	75±13	—	81±3	—	75±15	—
1.80	90±0	—	73±14	—	81±5	—	74±19	—
1.92	90±0	—	73±14	—	79±7	—	74±19	—
2.04	90±0	—	72±13	—	81±6	—	84± 9	—
2.16	90±0	—	77±13	—	84±3	—	86± 6	—
3.24	90±0	—	73±12	—	85±0	—	90± 0	—
4.32	90±0	—	72±11	—	87±2	—	90± 0	?

deviations in this case also increase as the temporal interval increases. These two *O*s do not report a single case of simultaneous localization of sounds on both sides, as they do in the previous experiment.

Localization for *L* and *M* is more or less irregular, but the irregularities are so distributed that they show no general tendency toward medial localization. These *O*s sometimes thought (they were not sure and entered '?' in their record) they heard sounds on both sides, and occasionally they reported double lateral localization.

It appears, therefore, that temporal differences apposed with a relatively large intensive difference cause slight irregularities in the localization, but that such differences do not introduce a double lateral effect, do not equalize the 'effect' of the intensive difference, and do not condition localization as do intensive differences when apposed with a relatively large temporal difference.

(*5*) *Localization with temporal and intensive differences.* The last experiment of this series dealt with localization as a function of temporal and intensive differences presented together in the same direction. The purpose was to determine if these factors supplement each other in conditioning directional perception.

Results. Table V shows the results. It is evident from a comparison of this table with Tables I and II that angular displacement increases much more rapidly in the earlier stages of

TABLE V

LOCALIZATION AS A FUNCTION OF TEMPORAL AND INTENSIVE DIFFERENCES PRESENTED TOGETHER

Intensive ratio	Temporal difference	Observers			
δ	σ	*H*	*Hd*	*L*	*M*
1.00	0.00	X	X	X	X
1.13	0.12	25±36	11± 8	16± 6	4± 4
1.27	0.24	26±31	40±20	40±24	30±20
1.42	0.36	78±19	50±28	54±27	48±18
1.60	0.48	87± 5	56±20	52±18	58±24
1.80	0.60	78±18	50±15	52± 9	65±24
2.03	0.72	62±21	52±16	52± 4	38±18
2.28	0.84	42±21	60±12	54± 7	45±24
2.57	0.96	63±17	71±13	61± 7	50±24
2.87	1.08	58±19	74±11	58± 7	52±18
3.25	1.20	75± 9	85± 2	71±13	74±17
4.11	1.32	84± 5	89± 2	79± 2	84± 9
5.20	1.44	89± 1	89± 2	82± 4	88± 3
6.57	1.56	80± 0	87± 2	84± 3	90± 0
8.32	1.68	90± 0	87± 2	86± 3	90± 0
10.53	1.80	90± 0	86± 3	88± 2	90± 0

the combined series than it does when either series is presented alone. The angular displacement increases very rapidly in the first three or four steps of the series, after which there is a plateau effect over the middle range, with a sudden swing to the maximal lateral position at the end of the series. The sound comes to the maximal lateral position slightly sooner in the combined series than it does when the intensive series is presented alone, and much sooner than it does when the temporal series is presented alone.

The average deviations indicate that localization is more difficult, more indefinite and uncertain, than it is when conditioned by a single factor.

It appears, therefore, that the intensive and the temporal factors supplement each other in determining directional localization.

III. Summary and Conclusions

Localization with intensive differences is continuous and fairly uniform. The 'intensity-effect' may be described in much the same manner as is either the 'phase-effect' or the 'time-effect,' *i.e.* as corresponding more or less to a semi-circle, a 90° arc on either side of the median plane.

Intensive differences not only neutralize the 'effect' of a relatively large temporal difference; they also either introduce a double lateral effect or condition approximately the normal 'intensity-effect.'

Temporal differences cause slight irregularities in localization conditioned by a relatively large intensive difference, but such differences do not introduce a double lateral effect, do not equalize the 'effect' of the intensive difference, and do not otherwise condition localization, as do intensive differences when apposed with a relatively large temporal difference.

The temporal and the intensive factors supplement each other in their effect on localization.

More briefly, the results seem to indicate (1) that, while both time and intensity as physical variants condition localization, these factors do not function in the same manner as conditions of auditory stimulation; and (2) that intensity may be the more effective factor in determining directional perception in auditory space.

IV. Theoretical Interpretation

Since the results of this analysis indicate that intensive differences are very significant conditions in auditory directional perception, these results may be interpreted as supporting the intensity-theory of sound localization. But whether intensity is the main determining factor throughout the entire pitch (frequency) range of auditory stimuli remains to be demonstrated.

That time-differences are not reducible to intensity-differences is fairly evident. Boring has advanced the theory that, because of inhibitory processes, the cortical excitation is greater for the prior stimulus, and that a temporal difference is therefore reduced to an intensive difference at the auditory center; *i.e.* the prior stimulus to a greater or less degree, depending on the temporal interval, inhibits the later.[20] If this theory held, it seems that temporal differences apposed with a relatively large intensive difference should be effective in the same manner as are intensive differences apposed with a relatively large temporal difference. But the results indicate that they are not. This fact fails to support the interpretation of the intensity-theory that reduces phase and time to intensity.

The results can not be considered as supporting the phase-time-intensity-theory because each factor, when the other factor was controlled, was found to condition its normal 'effect' with high-pitched sounds used as stimuli.

Since the intensive differences were found to be more effective than temporal differences in determining localization when high-pitched sounds were used as stimuli, and since these factors were found to supplement each other in conditioning localization, and, further, since intensive differences have been found to be effective, though not as effective as phase differences (defined as the time-difference in the arrival of corresponding phases at the ears), in ranges of relatively low frequency,[21] it seems that the results of the present investigation may best be interpreted in the light of the 'difference-pattern-theory.'

Intensity may be the more effective factor in determining localization under the conditions of the present investigation, but time may be the more effective factor under other conditions, while both factors may contribute, in the degree that they are present in the complex conditions of hearing, to a binaural difference-pattern that results in directional perception.

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[20] E. G. Boring, *op. cit.*, 168 f.

[21] This conclusion is drawn from the investigations of Halverson and Banister, as reviewed in a previous section of this report.

THE RELATION OF HETEROGENEOUS AND HOMOGENEOUS CHROMATIC STIMULI IN THE RANGE OF VISUAL APPREHENSION EXPERIMENT

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The present paper is a continuation of the series of studies in the 'range of visual apprehension' initiated by Fernberger's study[1] in 1921, where he, as Dallenbach[2] in 1920, came to the conclusion that the classical 'range of attention' experiment is not a problem in attention but rather one in apprehension. The conscious patterns which exist in this type of experiment were first analyzed and characterised by Oberly[3] and further investigated by Cooper[4] and Hart.[5] The appropriateness of the terms used by Oberly and adopted in the succeeding papers to denote these patterns has been disputed, but the phenomena described have consistently been corroborated.[6] The recent study of Glanville and Dallenbach,[7] in addition to its verification of Oberly's analyses, contains a very complete historical account of the development of the 'range of attention' experiment and introduces a more neutral terminology than that previously employed. The terminology of the present paper conforms to that of Glanville and Dallenbach.

The previous studies from the University of Pennsylvania have already determined experimentally (1) the interrelation of the statistical limens of the three conscious patterns (Oberly), and

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[1] S. W. Fernberger, A preliminary study of the range of visible apprehension, this Journal, 32, 1921, 133.

[2] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3 1920, 229.

[3] H. S. Oberly, The range for visual attention, cognition, and apprehension, this Journal, 35, 1924, 332-352.

[4] S. F. Cooper, The effect of brightness in the range of attention experiment, *ibid.*, 40, 1928, 254-274.

[5] J. R. Hart, The range of visual attention, cognition, and apprehension for colored stimuli, *ibid.*, 40, 1928, 276-283.

[6] K. M. Dallenbach, Dr. Oberly on the 'range of visual attention, cognition, and apprehension', *ibid.*, 36, 1925, 154-156; Dr. Fernberger on the 'range of attention' experiment, *ibid.*, 38, 1927, 479-481; S. W. Fernberger, The 'range of attention' experiment, *idem.*, 478-479.

[7] A. D. Glanville and K. M. Dallenbach, Range of attention, *ibid.*, 41, 1929, 207-236.

the effect (2) of brightness (Cooper), and (3) of color (Hart) on these limens. The last was, however, concerned with only homogeneous chromatic stimuli. The present study considers both homogeneous and heterogeneous chromatic stimuli and their interrelations.

Summarily we may define, then, the problem of the present paper as an investigation of the effect of homogeneous and heterogeneous chromatic stimuli on the statistical limens of the three conscious patterns which Glanville and Dallenbach have called 'Unit,' 'Group,' and 'Mediate' Apprehension, and which correspond descriptively to those patterns designated, respectively, by Oberly as 'Attention,' 'Cognition,' and 'Apprehension.'

Experimental Procedure

The stimulus-objects used in this experiment were colored circular dots of the four primary colors on a white background. The dots, 6.5 mm. in diam., were cut from the Hering papers—saturated yellow, blue, red, and green—and were pasted on white cards, 87 mm. sq. The stimulus-cards were grouped into 7 different stimulus-series—a 'series' being a set of 12 cards, on each of which there were a different number of dots. The smallest number of dots used was 3, and the greatest number was 14. These stimulus-series were, further, classified into two different groups, a 'homogeneous,' and a 'heterogeneous' group. The homogeneous group consisted of those series the cards of which bore stimulus-dots all of a single color; the heterogeneous group consisted of those series the cards of which bore stimulus-dots of two or more colors. The homogeneous group comprised (1) series Y, in which the stimulus-dots were all yellow; (2) series B, in which the stimulus-dots were all blue; (3) series R, in which they were all red; and (4) series G, in which they were all green. The heterogeneous group comprised (1) series Y-B, any card of which contained one or more blue and one or more yellow stimulus-dots; (2) series R-G, any card of which contained one or more red and one or more green stimulus-dots; and (3) series R-G-Y-B, any card of which contained one or more stimulus-dots of each of the four primary colors.

The manner of pasting the dots on the cards was, to begin with, haphazard. That is to say, the appropriate number was placed upon each of the 12 cards of one series at random. An accidental configuration resulted in the case of each number, which was reproduced in all the series. Thus there was a 3-dot pattern, a 4-dot pattern, and so on up to a 14-dot pattern; and these patterns were repeated in all the series of both the homogeneous and the heterogeneous groups. The form-quality of the stimuli was, in this way, kept constant.

In the heterogeneous group, a question arose in regard to the distribution, on the same card, of the two or more colors. The method adopted was as follows. The colors of each of the stimulus-series R-G, Y-B, and R-G-Y-B were repeated in succession an indefinite number of times forming three definite sequences: R-G-R-G-R-G etc. in the first series; Y-B-Y-B-Y-B etc. in the second series; and R-G-Y-B-R-G-Y-B etc. in the last series. From each of these the first three colors were taken to construct the 3-dot card in the respective series; the next 4 colors were taken to construct the 4-dot card; and so on, until the 12 cards of each of the series were constructed. In the two-color series, series R-G and Y-B, the red and green and the yellow and blue were distributed equally on the cards containing an even number of dots, and unequally distributed on the cards containing an odd number of dots. In series R-G, the 3-, 7-, and 11-dot stimulus-cards contained one more red dot than green, and the 5-, 9-, and 13-dot cards, one more green dot than red. In series Y-B, the 3-, 7-, and 11-dot cards contained one more yellow dot than blue, and the 5-, 9-, and 13-dot cards, one more blue than yellow dot. In series Y-B, the yellow dots were given the position of the red dots, and the blue dots that of the green dots, of series R-G. This procedure was adopted so that there would be a greater evenness of color distribution in the series as a whole, the yellow and green pigment papers having a higher luminosity than the blue and red. In the four-color series, the colors were distributed equally only on the 12-dot stimulus-card, there being on this card 3 dots of red, three of green, three of yellow, and three of blue. As far as possible the different colors were distributed on the cards of this series so that the same color did not occupy contiguous positions.

The 7 series of 12 cards each were exposed in rotation by means of a Whipple tachistoscope,[8] the exposure time being the same as Oberly's,[9] 37.5 σ calibrated by the tuning-fork method. The order in which these 84 cards were exposed was haphazard, but no cards containing dots of the same color or color combination, or containing the same number of dots were exposed successively, except that 6-dot cards followed each other in one instance, and 7-dot cards followed each other in another instance. The position in which the cards were placed in the tachistoscope for the first exposure was recorded for each card. For the second exposure, the cards were rotated from this position 90°; for the third exposure, 180°; and for the fourth exposure, 270°. This manner of rotating the cards was followed throughout the experiment. For the fifth exposure, the cards were again in the first position. By following this method, the same card could appear in exactly the same position only after 336 exposures. In this way, and by terminating each period of observation at different places in the series so that a different card was exposed first at successive periods, the *O* was prevented from learning the order of exposure and from readily recognizing any particular card.

The experiments were conducted in a dark-room. The only light constantly present was that of the exposure lamps, thus keeping constant the illumination of the stimulus cards and the general illumination. *O* sat 1.9 m. from the exposure screen. He was instructed to look at the fixation-point on this screen at the signal word 'Ready.' A second later, the experimenter released the weighted arm of the tachistoscope, simultaneously giving the second signal word 'Now,' and the stimulus-card was exposed automatically. *O* then gave his report verbally, as quickly as possible, according to instructions which will be described below, and *E* immediately recorded it. The *O*s were given practice in following these instructions before any judgment was recorded, and they were allowed to look at the instructions at the beginning of successive periods of observation whenever they so desired.

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[8] G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1924, 264 ff.

[9] Oberly, *op. cit.*, 334.

Observers. We used 5 *O*s—*A*, *G*, *M*, *N*, and *R*, all with scientific training. None of them had acted as *O*s in the earlier experiments mentioned above, but *A* and *G* were familiar with the type of work and the general experimental procedure. All the *O*s had, however, sufficient laboratory practice to put them at ease in the experimental situation of this problem and to enable them to accommodate quickly and adequately to the special procedures that we used.[10]

Instructions. The following instructions, essentially the same as those used by Oberly,[11] were given the *O*s.

"You will be shown successively, for a very short exposure, cards which contain a varying number of dots on a white background. Immediately after the exposure you will report verbally (1) the number of dots you have seen; (2) the degree of assurance of certainty of your judgment, using the following scale for your report: 5—if it is a 100 to 1 bet, 4—if it is a 5 to 1 bet, 3—if it is a 1 to 1 bet, 2—if it is a 1 to 5 bet, and 1—if it is a guess or if you do not know; (3) the method of determining your report—whether by 'immediate' judgment (*i.e.* ungrouped), by 'grouping,' or by 'counting.' 'Immediate' means that the dots are perceived in a single flash of attention and were ungrouped. 'Grouping' means that the dots are seen in a single flash of attention but were grouped. 'Counting' means that the report is made on the basis of some re-imaging of the stimulus-material, *i.e.* some mediating conscious process is used and the dots are counted in one way or another."

Two of the *O*s (*N* and *R*) returned 50 judgments for every one of the 84 stimulus-cards, and the other 3 (*A*, *G*, and *M*) returned 100 judgments for every card. The different series of cards were exposed on an average from 3 to 4 times in an observational hour. During the early stages of the experiment not so many exposures were made, but during the later stages more exposures were possible because the 3-dot cards were dropped when the *O*s, due to practice, invariably reported 4- and 5-dot cards correctly, and the 4-dot cards of the homogeneous series (except the yellow dot card) were dropped when the *O*s invariably reported 5- and 6-dot cards correctly. In the cases of 2 of the *O*s (*M* and *R*) all but three of the 5- and 6-dot cards (those of Series Y, Y-B, and R-G-Y-B) were dropped, late in the experiment, when the 7- and 8-dot cards were invariably reported correctly.

The average length of the observational periods was one hour, and the *O*s usually observed three periods a week. The *O*s were permitted frequent rests. In the cases of the *O*s who returned 100 judgments for every stimulus-card, an interval of at least a week lapsed between the first and second series of 50 judgments. The experimental work was begun in the winter of 1927 and completed in the summer of 1928.

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[10] I wish to express my appreciation of the splendid coöperation of my *O*s, and for their industry throughout the experimental period. My thanks are also due Professor Fernberger who suggested the problem and under whose direction it was carried out. His advice and criticism were both a help and an encouragement.

[11] Oberly, *op. cit.*, 335. Cf. also S. F. Cooper, *op. cit.*, 257; Hart, *op. cit.*, 276.

Results

In the following tables, the data based upon the judgments characterized as 'immediate' by the *O*s are classified under 'unit' apprehension; these data plus those based on the judgments characterized as 'grouped' are classified under 'group' apprehension; and these two types plus the data based on judgments characterized by the *O*s as 'counted' are classified under 'mediate' apprehension.[12] Judgments that were given with an assurance of less than '3,' or that did not give the correct number of dots exposed, were not considered in the statistical treatment of the results.

The observed relative frequencies. An examination of the percentages of correct judgments with an assurance of '5,' '4,' or '3' shows that the number of dots apprehended increases from 'unit' apprehension to 'group' and to 'mediate.'

A comparison of the results of the first and second series of 50 judgments—for those *O*s who returned 100 judgments for every stimulus-card—shows that the percentages correctly reported are greater for the second than for the first set. The increase is especially noticeable in 'group' and 'mediate' apprehension, particularly in the data of *G* and *M*. It is conspicuously apparent in the reports on stimulus-cards containing from 3 to 5 dots.

The limens. The statistical limens and measures of precision shown in Table I were obtained from the data by the method of constant stimuli. The limens of every stimulus-series increase, as a rule, from 'unit', to 'group', to 'mediate' apprehension.

The limens derived from the second series of 50 judgments show the effect of practice. They are greater in magnitude than those derived from the first, and the increase is largest for 'mediate' apprehension, less large for 'group' apprehension, and smallest for 'unit' apprehension.

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[12] For an explanation and justification of this terminology see K. M. Dallenbach, Dr. Oberly on 'the range for visual attention, cognition and apprehension', this JOURNAL, 36, 1925, 155; and Glanville and Dallenbach, *op. cit.*, 220 f.

TABLE I

Showing the Limens (L) and the Measures of Precision (h) for Every One of the Stimulus-Series for Every Cognitive State

O	Stimulus-series	'Unit' apprehension		'Group' apprehension		'Mediate' apprehension	
		L	h	L	h	L	h
	Y	6.2	0.62	7.4	0.38	7.5	0.35
	B	6.2	1.02	7.3	0.33	7.4	0.31
	R	6.6	1.18	7.1	0.32	7.2	0.33
A	G	6.3	1.11	7.6	0.27	7.7	0.29
	R-G	6.8	0.44	7.1	0.38	7.1	0.34
	Y-B	5.1	0.51	7.2	0.27	7.3	0.40
	R-G-Y-B	6.4	0.78	7.2	0.36	7.2	0.23
	Y	5.8	0.37	6.1	0.32	6.7	0.24
	B	5.5	2.56	7.4	0.69	8.5	0.66
	R	5.1	2.11	7.3	0.74	8.6	0.61
G	G	5.7	4.23	7.4	0.49	8.5	0.27
	R-G	5.3	1.44	7.0	0.54	8.5	0.48
	Y-B	4.2	0.69	5.0	0.38	5.4	0.33
	R-G-Y-B	5.1	0.43	5.4	0.40	5.8	0.32
	Y	7.3	0.15	7.8	0.23	8.1	0.25
	B	8.8	0.37	8.1	0.47	9.7	0.26
	R	7.9	0.53	8.3	0.62	9.6	0.30
M	G	8.2	0.50	7.9	0.64	9.8	0.26
	R-G	8.2	0.08	8.2	0.08	9.8	0.26
	Y-B	6.7	0.56	6.8	0.48	8.4	0.28
	R-G-Y-B	6.8	0.49	6.9	0.47	9.4	0.27
	Y	5.3	0.47	6.1	0.35	7.0	0.28
	B	6.6	1.03	7.2	0.77	9.1	0.35
	R	6.8	1.71	7.3	1.04	9.0	0.29
N	G	6.7	1.29	7.5	0.76	9.1	0.35
	R-G	6.5	0.66	6.3	0.47	8.4	0.32
	Y-B	6.2	1.02	6.2	0.58	7.1	0.31
	R-G-Y-B	6.2	0.66	6.6	0.59	7.9	0.37
	Y	6.0	0.43	7.7	0.30	10.1	0.21
	B	7.2	0.46	8.6	0.33	10.4	0.11
	R	7.0	0.51	8.8	0.33	12.0	0.18
R	G	6.8	0.46	9.0	0.30	11.8	0.20
	R-G	6.6	0.47	8.4	0.37	10.2	0.19
	Y-B	6.2	0.70	6.8	0.33	8.7	0.15
	R-G-Y-B	6.7	0.53	8.0	0.30	10.2	0.20

The rank orders of the different stimulus-series. Table II, derived from Table I, shows the rank orders of the limens of the different stimulus-series based upon the first set of 50 judgments. Rank 1, or first place, was given to the series whose limen was greatest in magnitude; rank 2, or second place, was given to the series with the next greatest limen; and so on to rank 7, or last place, which was given to the series whose limen was smallest in magnitude.

The data given in Table II were cast in the form of the scattergrams (Table III) which in turn were used as the basis of the final classification of the stimulus-series shown in Table IV.

The rank order given in Table IV was derived from Table III as follows: for every stimulus-series the sum of its rank orders 1–4 was compared with that of its rank orders 4–7. If the former was greater it was placed in the 'high' group; if the latter was greater it was placed in the 'low' group; and if the sums were approximately equal it was placed in the 'medium' group. It will be observed that the final order for the different stimulus-series is, with one exception, the same for every cognitive pattern. The exception occurs in 'unit' apprehension in which the stimulus-series occupying ranks 5 and 6 are the reverse of the ranks occupied in the other two cognitive patterns.

TABLE II

THE RANK ORDER OF THE STIMULUS-SERIES BASED ON THE MAGNITUDE OF THEIR RESPECTIVE LIMENS FOR EVERY *O* AND FOR EVERY COGNITIVE PATTERN

Cognitive pattern	Rank order	Observers				
		A	*G*	*M*	*N*	*R*
'Unit' apprehension	1	R-G	Y	B	R	B
	2	R	G	R-G	G	R
	3	R-G-Y-B	B	G	B	G
	4	G	R-G	R	R-G	R-G-Y-B
	5	B	R-G-Y-B	Y	R-G-Y-B	R-G
	6	Y	R	R-G-Y-B	Y-B	Y-B
	7	Y-B	Y-B	Y-B	Y	Y
'Group' apprehension	1	G	G	R	G	G
	2	Y	B	R-G	R	R
	3	B	R	B	B	B
	4	R-G	R-G	G	R-G-Y-B	R-G
	5	R-G-Y-B	Y	Y	R-G	R-G-Y-B
	6	R	R-G-Y-B	R-G-Y-B	Y-B	Y
	7	Y-B	Y-B	Y-B	Y	Y-B
'Mediate' apprehension	1	G	R	G	B	R
	2	Y	G	R-G	G	G
	3	B	B	B	R	B
	4	Y-B	R-G	R	R-G	R-G
	5	R-G-Y-B	Y	R-G-Y-B	R-G-Y-B	R-G-Y-B
	6	R	R-G-Y-B	Y-B	Y-B	Y
	7	R-G	Y-B	Y	Y	Y-B

The stimulus-series were similarly classified into 'high,' 'medium,' and 'low' upon the basis of the average limens of all the *Os*. These data appear in Table V. The rank orders given in Table IV may be called the 'preferential' orders, while those given in Table V may be called the 'statistical' order. The orders in both instances are, however, substantially the same; only five points of disagreement occur—two in 'unit' apprehension, one in 'group' apprehension, and two in 'mediate' apprehension—and they consist of the reversal of neighboring rank orders.

The classification of the stimulus-series into 'high,' 'medium,' and 'low' is, with one exception, the same in the two tables. The exception occurs in 'unit' apprehension in which the rank orders of series R and R-G are reversed. The numerical difference between these two series is, however, so small—0.024—that the discrepancy may be said not to affect the general tendency shown by these tables. The highest limens—in order of preference—in all of the cognitive patterns for all of the *Os* collectively are stimulus-series G, B, and R; the medium limen is R-G; and the lowest limens are Y, R-G-Y-B, and Y-B.

TABLE III

Scattergrams of the Rank Order of Every One of the Seven Stimulus-Series for Every Cognitive Pattern

Cognitive pattern	Rank order	Stimulus-Series						
		Y	B	R	G	R-G	Y-B	R-G-Y-B
'Unit' apprehension	1	1	2	1	0	1	0	0
	2	0	0	2	2	1	0	0
	3	0	2	0	2	0	0	1
	4	0	0	1	1	2	0	1
	5	1	1	0	0	1	0	2
	6	1	0	1	0	0	2	1
	7	2	0	0	0	0	3	0
'Group' apprehension	1	0	0	1	4	0	0	0
	2	1	1	2	0	1	0	0
	3	0	4	1	0	0	0	0
	4	0	0	0	1	3	0	1
	5	2	0	0	0	1	0	2
	6	1	0	1	0	0	1	2
	7	1	0	0	0	0	4	0
'Mediate' apprehension	1	0	1	2	2	0	0	0
	2	1	0	0	3	1	0	0
	3	0	4	1	0	0	0	0
	4	0	0	1	0	3	1	0
	5	1	0	0	0	0	0	4
	6	1	0	1	0	0	2	1
	7	2	0	0	0	1	2	0

The relation of heterogeneous to homogeneous stimuli. The distribution of the stimulus-series in Tables IV and V indicate the relative influence of homogeneous and heterogeneous stimuli on the *Os'* limens as a group. In every one of the cognitive patterns three of the four homogeneous stimulus-series make up the group of 'high' limens, and two of the three heterogeneous series fall within the group of the 'low' limens. Series Y, a homogeneous series, falls within the 'low' ranks, holding rank 6 in 'unit' apprehension, and rank 5 in 'group' and 'mediate' apprehension. Series R–G holds the fourth or 'medium' rank in all three of the cognitive patterns. From these data we may conclude that, under the conditions of our experiment, heterogeneity of stimuli gives, in most cases, a smaller limen than homogeneity. It is to be observed, however, that Y is a common factor in each member of the 'low' limen group, and, as we have seen, stimulus-series Y falls always within the 'low' order for all the cognitive patterns.

TABLE IV

RANK ORDER OF THE STIMULUS-SERIES FOR THE *O*S AS A WHOLE FOR EVERY COGNITIVE PATTERN, CLASSIFIED INTO GROUPS DESIGNATED 'HIGH,' 'MEDIUM,' AND 'LOW.' (FROM TABLE III)

Rank order		Cognitive patterns 'Unit' apprehension	 'Group' apprehension	 'Mediate' apprehension
	1	G	G	G
High	2	B	B	B
	3	R	R	R
Medium	4	R-G	R-G	R-G
	5	R-G-Y-B	Y	Y
Low	6	Y	R-G-Y-B	R-G-Y-B
	7	Y-B	Y-B	Y-B

TABLE V

RANK ORDER OF THE STIMULUS-SERIES FOR EVERY COGNITIVE PATTERN, BASED ON THE AVERAGE OF THE LIMENS OF ALL THE *O*S (From Table I)

Rank order		Cognitive patterns 'Unit' apprehension		'Group' apprehension		'Mediate' apprehension	
		Series	Av. L	Series	Av. L	Series	Av. L
	1	B	6·9	G	7·9	G	9·7
High	2	G	6·7	R	7·8	R	9·3
	3	R-G	6·7	B	7·7	B	9·0
Medium	4	R	6·9	R-G	7·4	R-G	8·8
	5	R-G-Y-B	6·3	Y	7·2	R-G-Y-B	8·1
Low	6	Y	6·1	R-G-Y-B	6·8	Y	7·9
	7	Y-B	5·9	Y-B	6·4	Y-B	7·4

It is also to be observed that the homogeneous and the heterogeneous stimulus-series holding the first four positions in the rank order for the *O*s as a whole are characterised by the absence of Y. From these facts, we may conclude that Y is an indispensable part of the cause of the low rank order of the heterogeneous stimuli. An experimental determination of this conclusion could be made by running additional two-color series, Y-R and Y-G, and three-color series, Y-R-G, Y-B-G, and Y-B-R.

The relation of the heterogeneous and homogeneous stimuli, in the case of every *O*, is shown quantitatively by Table VI. The statistical limens of each of the *O*s for the four homogeneous series and for the three heterogeneous series in each of the cognitive patterns were averaged, and the difference between these averages was taken. In every instance, the average heterogeneous limen is smaller than the homogeneous limen. If a difference of 0.5 or more is taken to be significant, there is a significant difference in 9 of the 15 cases (60%), and this difference increases from 'unit' to 'group,' to 'mediate' apprehension. For one *O*, *G*, there is a significant difference in each of the three cognitive patterns; for two *O*s, *R* and *N*, there is a significant difference in 'group' and 'mediate' apprehension; and for *M* there is a significant difference in 'unit' and 'group' apprehension. For *A* the difference is not significant in any of the cognitive patterns. We conclude from these data, therefore, that the range of visual apprehension for heterogeneous chromatic stimuli compared with that for homogeneous chromatic stimuli is significantly reduced.

This narrowing of the range of apprehension for heterogeneous stimuli is caused, in part, by heterogeneity, but the presence of Y is, we found, also part of the cause. Table VI, however, does not show to what extent Y is a causal factor, because Y appears in both the homogeneous and the heterogeneous groups and, therefore, affects both the homogeneous and the heterogeneous average limens. In order to show specifically the effect of Y, Table VII was constructed. The numbers, in Table VII, designated as 'high' are the averages of the three highest limens, those of stimulus-series G, B, and R (Table IV), and those designated as 'low,' the averages of the lowest limens, those of series Y, R-G-Y-B, and Y-B; series R-G was omitted from these calculations because

TABLE VI

The Average Limens of the Homogeneous and of the Heterogeneous Stimulus-Series and Their Differences for Every *O* for Every One of the Cognitive Patterns

O	'Unit' apprehension			'Group' apprehension			'Mediate' apprehension		
	Homo.	Hetero.	Dif.	Homo.	Hetero.	Dif.	Homo.	Hetero.	Dif.
A	6.3	6.1	0.2	7.4	7.2	0.2	7.4	7.2	0.2
G	5.5	4.9	0.6	7.1	5.8	1.3	8.1	6.2	1.9
M	8.0	7.3	0.7	8.0	7.3	0.7	9.4	9.2	0.2
N	6.4	6.3	0.1	7.2	6.3	0.7	8.5	7.8	0.7
R	6.8	6.5	0.3	8.5	7.7	0.8	11.0	9.7	1.3

TABLE VII

The Average Limens of the 'High' and of the 'Low' Stimulus-Series and Their Differences for Every *O* for Every one of the Cognitive Patterns

O	'Unit' apprehension			'Group' apprehension			'Mediate' apprehension		
	High	Low	Dif.	High	Low	Dif.	High	Low	Dif.
A	6.3	5.9	0.4	7.4	7.3	0.1	7.4	7.3	0.1
G	5.4	5.1	0.3	7.4	5.5	1.9	8.5	5.9	2.6
M	8.3	6.9	1.4	8.1	7.1	1.0	9.9	8.6	1.3
N	6.7	5.9	0.8	7.3	6.3	1.0	9.0	7.3	1.7
R	7.0	6.3	0.7	8.8	7.5	0.3	11.4	9.6	1.8

it gave the medium limen throughout. By this method Y was eliminated entirely from the set of averages designated as 'high,' and was a factor in every one of the series whose limens were averaged for the set designated as 'low.' In this way, then, the influence of Y on the thresholds may be more specifically shown.

The difference, for the majority of *O*s, between the 'high' and the 'low' average limens (Table VII) is greater than that between the homogeneous and heterogeneous average limens (Table VI)—the difference in the former case being less than 0.5 only four times, while in the latter case it is less six times, out of fifteen. In other words, the effect of Y is significant in 73.6% of the instances, while the effect of heterogeneity was, we saw, significant in only 60% of the instances. At least one significant difference is to be noted in Table VII for every *O*: *M* and *N* show significant differences in all three of the cognitive patterns; *R* and *G* in two of the patterns; and *A* in one. It is obvious from these data, then, that for our group of *O*s, as a whole and under our experimental conditions, Y reduces the range of visual apprehension for chromatic stimuli more, and to a greater extent, than does heterogeneity of stimuli.

Table VIII gives quantitative corroboration to the general conclusions that followed from the data given in Tables IV and V, namely, that the R-G limens differ least from the three high homogeneous limens, the Y-B limens differ most from the homogeneous limens and the other heterogeneous limens, and the Y limens differ very little from the low heterogeneous limens. If we consider the average differences, tabulated in the last column of Table VIII, for each cognitive pattern, we observe, in the first place, that the average limens for R-G do not differ significantly from the average of all the homogeneous limens, the R-G average limen being only 0.105 larger in 'unit' apprehension, 0.199 smaller in 'group' apprehension, and 0.070 smaller in 'mediate' apprehension, than the average homogeneous limen. In the second place, the average limen for Y-B is significantly smaller in each cognitive pattern than the average limens of the homogeneous series as a whole, the Y-B limen being 0.900 smaller in 'unit,' 1.212 smaller in 'group,' and 1.499 smaller in 'mediate' apprehension than the average homogeneous limen. The average limen for R-G-Y-B is also significantly smaller than the average limens for the homogeneous stimulus-series as a whole in 'group' and 'mediate' apprehension, but not in 'unit.' In the third place, the average limen for stimulus-series Y is not significantly different from the average limens for the heterogeneous stimulus-series in any of the cognitive patterns: the limen for Y being but 0.092 larger in 'unit' apprehension, 0.128 smaller in 'group' apprehension, and 0.069 larger in 'mediate' apprehension.

The averages tabulated in the bottom row of Table VIII show the amount and the direction of the differences of the averages for the heterogeneous and the homogeneous limens for all the *O*s. The average heterogeneous limens differ significantly in every cognitive pattern from the average homogeneous limens with the exception of the case of stimulus-series Y. In 'unit' apprehension the average heterogeneous limen differs most from the average limen for stimulus-series B; in 'group' and 'mediate' apprehension it differs most from the average limens for series G. In general, the average differences when minus, *i.e.* when the heterogeneous limens are smaller than the homogeneous, increase from 'unit,' to 'group,' to 'mediate' apprehension.

TABLE VIII

THE AVERAGE OF THE DIFFERENCES OF EACH OF THE HETEROGENEOUS LIMENS AND EACH OF THE HOMOGENEOUS LIMENS FOR EACH COGNITIVE PATTERN FOR THE *O*s AS A WHOLE

(The plus and minus signs indicate that the average heterogeneous limen is larger or smaller, respectively, than the average homogeneous limen compared with it)

Cognitive pattern	Heterogeneous	Homogeneous				
		Y	B	R	G	Av.
'Unit' apprehension	R-G	+0.585	−0.168	+0.024	−0.023	+0.105
	Y-B	−0.437	−1.191	−0.998	−0.975	−0.900
	R-G-Y-B	+0.129	−0.623	−0.430	−0.407	−0.283
	Av.	+0.092	−0.661	−0.468	−0.468	
'Group' apprehension	R-G	+0.412	−0.364	−0.337	−0.506	−0.199
	Y-B	−0.596	−1.353	−1.365	−1.534	−1.212
	R-G-Y-B	−0.200	−0.937	−0.949	−1.118	−0.776
	Av.	−0.128	−0.885	−0.884	−1.053	
'Mediate' apprehension	R-G	+0.930	−0.016	−0.485	−0.711	−0.070
	Y-B	−0.478	−1.644	−1.893	−1.982	−1.499
	R-G-Y-B	−0.246	−0.900	−1.169	−1.259	−0.894
	Av.	+0.069	−0.853	−1.182	−1.317	

The conscious patterns. The *O*s were required by the instructions in this experiment to designate the method of determining their reports as 'immediate,' 'grouped,' or 'counted,' according to certain general characterizations given them. This was not as simple as we had been led by previous experiments to believe. All the *O*s experienced difficulty at times in deciding the method which served as the basis of their reports.

The first category, 'immediate,' appeared to be unequivocal. Whenever the number of dots was apprehended immediately in a single flash of consciousness the judgment was given promptly and without hesitation. A few times, however, *G* and *N* reported some difficulty in deciding whether the judgment was 'grouped' or 'immediate.'

The second category, 'grouped,' appeared to be equivocal. This term was, by definition, used to designate the conscious pattern when the stimulus was perceived immediately, the number of dots appearing with different degrees of clearness, that is, attentional grouping of the dots occurred. This method of determining the report occurred rarely and it was thought by most of the *O*s to be due to some factor in the stimulus, *e.g.* to the different colors, to the particular arrangement of the dots on the stimulus-cards, or to both.

M reported that the conscious pattern 'grouped' was rarely experienced. Of the recorded judgments of this *O*, only 16 of the 4200 judgments of the first set were characterized as 'grouped,' all of these being early in the experimental work; none of the second set of judgments was so characterized.

G reported that he "was sometimes in doubt whether the judgment was 'immediate' or 'grouped,' " and found it "difficult to know if the grouping was objectively given or did I do it myself." Most of the judgments of the 6-dot stimulus-cards were given as 'grouped' by *G* but he felt that this grouping was certainly due to the objective arrangement of the dots.

N, who gave a fair percentage of 'grouped' judgments, stated that "grouping by colors was the rule." He commented that "anything above a 5, that is, the 6s and 7s, usually were grouped."

A reported that "grouping depends on the position of dots on the cards." A judgment was called 'grouped' when the dots were "arranged in a number of separate groups." *A* gave the highest percentage of 'grouped' judgments; the psychometric functions of 'group' and 'mediate' apprehension, graphically presented, appear in his case almost to coincide.

The third category, 'counted,' appeared to be not as unequivocal as the first but less equivocal than the second. A report made on the basis of some subsequent, mediating, mental processes, involving some kind of re-imaging of the stimulus material, was called 'counted.' Most of the *O*s characterized the greater part of the judgments recorded in the third category, "grouped plus counting." They reported that the dots were divided into groups, wholly or in part, and the final sum was found by adding together the sum of the individual groups or, by adding together the sum of the groups and the remaining single scattered dots.

The introspections raise a question as to the causes, under the conditions of this experiment, of the variability of the categories in their application. Our experimental data do not, however, give us the material for an answer. Further experimental work is needed to determine what, under the conditions of our experiment, are the causes affecting the stability of these categories.

The effect of color on the range of visual apprehension. Hart investigated the effect of chromatic homogeneous stimuli on the range of visual apprehension and found that red gave the largest limen, green the smallest, and yellow and blue limens of intermediate size.[13] We found in this experiment that in general green gave the largest limen, yellow the smallest, and blue and red limens intermediate in size. In other words, the order of single colors based on the serially decreasing magnitude of the respective limens was found by Hart to be (1) red, (2) yellow, (3) blue, (4) green; while the order on the basis of our data was found to be (1) green, (2) blue, (3) red, (4) yellow. How may these differences be accounted for? The same set of chromatic papers were used in each experiment, and the stimulus-objects were dots. Hart, however, presented the dots on a gray background, while in the present study the dots were presented on a white background, the other conditions of the two experiments being the same—the pre-exposure field being black and the tachistoscope being operated in a dark-room. The difference in the background of the stimulus dots may or may not be the chief cause of the differences in the magnitude of the limens of the respective colors in the two instances, but, in either case, it must be considered part of the cause of the recorded differences.

According to the introspective notes, the *O*s in our experiment experienced difficulty in making judgments at certain times, if not all the time, on account of the characteristics of a single color or of the grouping of different colors, although each *O* remarked, at some time during the experiment, "color doesn't seem to make much difference."

In the case of single colors, yellow occasioned the greatest difficulty. This was due, it was reported by every *O* at some time, to the "faintness of the yellow," which made it "difficult to see the yellow." At the beginning of the experiment, when a card of stimulus-series Y was exposed, the *O*s reported "only a flash of yellow," or "nothing at all," or "just a bunch of yellow."

M reported at first exposure of the 8-dot card of series Y, "can't see anything at all;" and at the 37th exposure of the cards, "yellow always hard to get."

R remarked on the 7th exposure of the 4-dot card of series Y, "white hardest to see," on the 14th exposure of the 8-dot card, series Y, "can't see at all," and on the 17th exposure, 13-dot card, series Y, "can't see all the yellow dots.''

N stated, "I do most of my counting from a kind of after-image and yellow doesn't leave much of a memory image." At the 1st, 17th, 20th, and 42d exposures of some of the cards of series Y, he reported, "yellow, but I can't see the number, too faint."

For *G*, from the 1st to the 50th exposure, in both the first and second sets of judgments, the yellow dots were always a cause for some exclamation of disgust or displeasure, especially when more than 6 dots were exposed.

In the case of mixed colors, fewer difficulties were experienced but more were occasioned by Y than by the other single colors, R, G, and B. When cards of 8 dots and upwards of the four-color series R-G-Y-B were exposed several of the *O*s spoke of the "awful mixtures." The majority of the *O*s reported that the Y-B combination was the hardest, but it was the Y-factor that was mentioned as the most bothersome, here again, due to its faintness.

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[13] J. R. Hart, *op. cit.*, 283.

Thus, we may say that Y appears to have had, introspectively, more effect on the judgments of the number of stimulus dots apprehended than any other single color and any color combination.[14]

The dot-patterns and their configuration. A phenomenon occurred during the progress of the experiment which we shall call, for want of a better name, 'configuration of the dot-patterns.' By this term, we mean, the tendency on the part of the *O*s to perceive a certain number of stimulus dots as in a particular form or configuration and, then, consciously or unconsciously, to take hold of the particular configuration perceived as the 'sign' of the number of dots exposed.

For example, *A* reports, "after several trials a number of the arrangements of dots was nearly always the same and I could be sure of five even though I had not been able to perceive all the dots, just because of the arrangement, for six never appeared in certain of the arrangements which five always appeared in. With the larger number of dots, when a certain arrangement appeared time after time, I learned to give attention more definitely to a certain part of the dots, and as a consequence could even increase the number of dots which I could perceive."

G reported, "as the experiment goes on apperception plays an increasingly greater rôle, sometimes consciously, sometimes unconsciously. I've observed that I have been giving degrees of assurance in the latter part of the experiment that I couldn't have given before, take 7 for example, no matter how turned around, I always get it in groups of 3 and 4. I got to know the pattern; and if I didn't see all of the dots, due to lax attention, I'd fill it in. The manner of judging goes toward the 'immediate'-end by getting less 'grouped' and 'counted' and more just 'grouped.' Now I know how the patterns of 6 and 7 look. . . . and 10 equals three lines and a curly cue. I don't have to give my full attention, just a glimpse is sufficient."

At the beginning of the experimental work, however, the cards of the various series containing the same number of dots on different exposures appeared, to all the *O*s, to be different. *G*, for example, remarked that "even when there are the same number of dots, the pattern seems different and therefore it is sometimes easier and sometimes harder to judge the number;" and suggested that "the same number of dots in each series might have been arranged in the same pattern." It was not until later in the experiment that the *O*s perceived certain numbers as particular configurations.

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[14] The term configuration as used in this paper does not imply any opinion, on the part of the writer, as to the positions taken by the *Gestalt* School whose fundamental concept, *Gestalt* or *Struktur*, has, in English, been rendered by the term 'configuration.'

Spontaneous comments on the special configurations of certain numbers of dots were made earlier or later in the experimental work and at different times by the different *O*s. *M*, on the 8th exposure of the stimulus cards observed that a 7-dot card was "upside down," and on the 22d exposure greeted a 7-dot series B card as an "old friend," apprehending the number of dots 'immediately' (previously the number had been reported 'counted'). *N* on the 28th exposure stated that the 7- and 6-dot cards were "remembered." *R* on the 41st exposure remarked, after a 7-dot card had been exposed, "a bunch of sevens of this form." *G* was not sure of the 7-dot pattern in any of the series until the second set of 4200 judgments, that is, after, at least, 50 exposures. *A* made no specific comment on the 7-dot pattern in either the first or second set of judgments.

Such variability from *O* to *O* in the time of the beginning of configuration of the dots of the different stimulus-cards, as we have indicated in the case of 7 dots, was shown in the case of all the numbers of stimulus dots (that were configurated) above 5 or 6, and varied from series to series for any one *O*. Further, configuration seemed to reach an upper limit at different numbers of dots for different series, varying among the *O*s, the limit probably bearing a direct relation to the respective limens for 'unit' apprehension.

The factors upon which this configuration depends are not specifically given among the experimental data of the present investigation. Certain indications are, however, apparent. It may be held that configuration depends, in this experiment, upon the stimuli, since the dot-patterns were the same for all the series. But the manner of rotating the stimulus cards for each successive exposure, so that not before 336 exposures were made could the same card appear again, and the fact that cards with the same dot-pattern were not, at any time, exposed consecutively prevented the *O*s from easily learning the order of the exposure and readily recognizing the dot-patterns.

It is, no doubt, true that configuration, in our experiment, depends, in part, upon certain objective factors, for example, the arrangement of the dots upon the stimulus cards and the color or color-mixture of the dots. But it is also true that configuration depends, in part, upon certain subjective or psychosomatic factors, for our experimental data show that configuration began at different times in the progress of the experiment in the case of different *O*s, and that the extent and degree to which this phenomenon affected the judgments varied from *O* to *O*. It appears, therefore, that configuration depends, in part, upon the peculiar mental abilities and 'set,' of the individual *O*s. Thus, we may say that configuration is both a function of the stimulus and of the experiencing individual.

CONCLUSIONS

(*1*) The statistical limens for each of the three cognitive patterns for all the stimulus-series hold, in general, the following relations: the value of the limens for 'unit' apprehension are smallest in magnitude; those for 'mediate' apprehension, largest; and, for the most part, those for 'group' apprehension are intermediate.

(*2*) The interrelation of the statistical limens of the different series for each of the three cognitive patterns is, in general, as follows: the limens greatest in magnitude are given, on the average, by series G, B, and R; the limens intermediate in magnitude, by series R-G; and the limens smallest in magnitude, by series Y, R-G-Y-B, and Y-B.

(*3*) The color preference of the group of *O*s as a whole, indicated by the serially decreasing magnitude of the respective limens, is as follows: (1) G, (2) B, (3) R, (4) R-G, (5) Y, (6), R-G-Y-B, (7) Y-B. The order of the homogeneous series according to the decreasing magnitude of respective limens is (1) G, (2) B, (3) R, and (4) Y; and of the heterogeneous series, (1) R-G, (2) R-G-Y-B, and (3) Y-B.

(*4*) Heterogeneity of chromatic stimuli tends, under the conditions of this experiment, to reduce the magnitude of the limens for each of the three cognitive patterns, least for 'unit' and most for 'mediate' apprehension.

(*5*) The results of the present study indicate that yellow is an indisputable factor in the cause of this reduction of the range of visual apprehension by heterogeneity of chromatic stimuli, but what the exact rôle of yellow is cannot be determined under the conditions of the experiment and would require a special investigation.

(*6*) A comparison of the introspective notes of our *O*s indicated a certain degree of instability in the functioning of the three categories under the conditions of our experiment. The category 'immediate' appeared to be least equivocal, that of 'counted,' medially so, and that of 'grouped,' most equivocal.

(*7*) As the experiment progressed, and perhaps as the effect of practice, the stimulus-dots became configurated. The *O*s recognized the dots as having a definite form or pattern. This configuration affected the *O*s' judgments in two respects: (1) conscious momentary "wanderings of the attention" did not interfere with the apprehension of the correct number of dots; the recognition of their structure, sometimes accomplished in the periphery of consciousness, acted as a 'sign' of the number of dots; (2) the conscious pattern 'counted' disappeared in most cases after the configuration of certain stimulus-cards was established, and in these cases the conscious patterns 'immediate' or 'grouped' appeared instead.

The amount of practice required before this configuration began varied from *O* to *O*, and in the case of every *O* the number of dots which formed such a figure, within the limits set by this experiment, was relative to the statistical limen for the stimulus-series in question.

The specific factors upon which this configuration depends cannot be determined without further experimentation, but it appears to be both a function of the stimulus and of the experiencing individual.

EYE-MOVEMENTS AND THE PHI-PHENOMENON

By J. P. Guilford, University of Nebraska and
Harry Helson, Bryn Mawr College

One of the most common explanations advanced in the history of psychology for different kinds of visual phenomena has been the eye-movement hypothesis. It has been advanced to explain the perceptions of forms and symmetry, the visual illusions, the autokinetic sensation or Charpentier's illusion, illusions of reversible perspective, and many other facts. It has been revived lately as a possible explanation for a number of the phenomena observed in the perception of movement from stationary stimuli, or, as it has come to be known, the phi-phenomenon. While the eye-movement theory has proved to be inadequate, either as a necessary, sufficient or contributing condition, to explain all of the numerous cases to which it has been applied, it might appear that surely here, in the perception of movement from stationary stimuli, eye-movements must play a decisive rôle. It is not surprising, then, that the eye-movement theory should have been proposed for stroboscopic movement as early as 1886 by Fischer,[1] followed by Stern in 1894,[2] and lately by Higginson.[3] Higginson does not, however, offer it as a general condition underlying all seen movement, but merely as an assumption which most adequately explains a number of observations that he has made.

While the writers believe that a large number of facts in the literature (some of them reported by Higginson himself) effectually dispose of the eye-movement theory as an explanation of the phi-phenomenon, and that any theory must account for *all* the facts, not merely a few which seem to be explained by it, it seemed necessary to obtain direct experimental evidence of the part played by eye-movements during the perception of apparent

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*Accepted for publication March 16, 1929.

[1] O. Fischer, Psychologische Analyse der stroboskopischen Erscheinungen, *Phil. Stud.*, 3, 1886, 128-156.

[2] L. W. Stern, Die Wahrnehmung von Bewegungen vermittelst des Auges, *Zsch. f. Psychol.*, 7, 1894, 321-386.

[3] G. D. Higginson, The visual apprehension of movement under successive retinal excitations, this Journal, 37, 1926, 63-115; The place of ocular movements in stroboscopic perceptions, *ibid.*, 408-413.

be given in succession just once, a mode of presentation we shall call single observing (S.O.) (*Einzelbeobachtung*), or they could be repeated, a pair on every rotation of the phonograph disk, a mode of presentation which we shall call multiple observing (M.O.) (*Dauerbeobachtung*). We shall use the abbreviations, S.O. and M.O. respectively, for these two types of presentation. The same throw switch opened or closed the circuit to the signal-light simultaneously with that for the stimulus-lights. The second special arrangement consisted of a commutator switch by which the order of stimuli could be reversed. We were thus able to determine the direction of movement, either from right to left (R–L), or from left to right (L–R). This change prevented the development of definite expectations on the part of the *O*s, thus preventing anticipatory movements as well as strong figural after-effects.

Observers. The *O*s were W. E. Walton (W), assistant in psychology; K. W. Braly (B), and Edwin Newman (N), graduate students in psychology, and the writers, (G) and (H). The work was performed at the University of Kansas during the spring and summer sessions of 1928.

Procedure. The procedure in the preliminary experiments was to adjust the speed of the phonograph disk until *O* reported optimal movement, and then to present series of stimuli while his eye-movements were being photographed. He was instructed to maintain his fixation on the red fixation-spot. The preliminary results were immediately convincing, for very rarely were there significant eye-movements simultaneous with the presentation of the stimuli. It was difficult, however, to obtain a time relationship for the stimuli which would give phenomenal movement 100% of the time. On the other hand, there were eye-movements along with some stimuli and not with others. We could conclude that eye-movements are not absolutely essential for the perception of the phi-phenomenon but we could not say that they had nothing to do with apparent movement. It was necessary, therefore, to distinguish between those stimuli which gave apparent movement and those which did not. To this end we gave *O* a telegraph-key in circuit with the signal light and asked him to report by pressing the key whenever he saw movement. With considerable practice the *O*s were able to do this with sufficient accuracy for our purposes. These reports may be seen in Plate I as heavier and longer dots and dashes following (below) each pair of stimuli in the S.O. series, and after every group of 5 pairs in the M.O. series. In the latter series such a report means that *O* saw movement in more than half of the group of 5. Those who have observed in this kind of experiment will know how difficult it is to decide in a moment's time whether or not movement occurred. The various stages, succession, part movement, full movement, dual part movement, and simultaneity merge so gradually into one another that it is hard, even with plenty of time, to name the class to which any single case belongs. From the protocols of the *O*s we are probably safe in assuming that a report of movement means a full movement, since in our preliminary experiments we determined the optimal conditions for full movement. Some of the reported movements may, to be sure, include some dual part movements, but since Higginson explains all these cases by his eye-movement theory, they should properly be included with the full movements.

A single series of observations consisted of forty stimulus-pairs presented in S.O. and then forty pairs presented in M.O. and arranged as follows so far as order of stimuli is concerned: 5 R–L, 10 L–R, 10 R–L, 10 L–R, and 5 R–L. This arrangement was not adhered to strictly, a fact which helped further to prevent anticipation on the part of the *Os*. Two, and sometimes 3, such series were presented in a single experimental hour with rests between them. In the later experiments the speed of the disk was adjusted so as to give full movement about half of the time, and something nearer succession the rest of the time. By doing this we had identical physical conditions, but sometimes the *Os* got phenomenal movement and sometimes not. If eye-movements are responsible for the apparent movement, or have anything to do with it, they should be present in great numbers in the former case as compared with the second case. We have counted the number of eye-movements in the case of 3 *Os* who served in the later experiments when phenomenal movement was reported and when it was not, as will be seen in Table I.

Results

Table I shows the number of eye-movements which occur along with those stimuli which give phenomenal movement (first column) and those which do not (third column) during both the S.O. and M.O. presentations. The differences are very small, rarely, if ever, greater than the probable error of the difference, and they are negative as well as positive. There is absolutely no correlation between phenomenal movement and eye-movements. In fact, there seems to be a better chance for phenomenal movement, during the S.O. series, when the eyes do not move than when they do. This agrees with the observation of G that following the stimuli voluntarily with the eyes tends to destroy the phi-phenomenon and to give a rather 'unformed' configuration. But even in the S.O. series the negative differences are too small to be significant.

TABLE I

Percent of Stimuli Giving Movement Accompanied by Eye-movements

O		Movement Reported		No Movement Reported	
		Percent	p.e.	Percent	p.e.
G	S.O.	14.0	3.1	16.1	4.5
	M.O.	14.3	4.5	—	—
N	S.O.	7.3	3.3	12.8	2.6
	M.O.	15.7	3.4	11.1	3.5
B	S.O.	31.8	3.0	34.6	4.5
	M.O.	31.2	3.5	25.6	4.6

We next present the average duration of stimuli (*e*) and time interval between stimuli (*p*) which gave optimal movement for the different *O*s, in Table II. While the interval *p* may seem long for optimal movement, in view of the value of 60σ given by Wertheimer as the interval for best movement for most of his *O*s, it must be remembered that *p* depends not only upon *e* but also upon the distance between the stimuli and the intensity of the stimuli. According to Korte's laws,[5] there is an inverse relationship between the intensity of the stimuli, *i*, and the time interval, *p*, necessary for apparent movement. Since our stimulus lights, whose intensity was controlled by means of a rheostat, were adjusted at a rather low intensity, we should expect the interval *p* to be greater than it might well be under different conditions.

TABLE II

SHOWING IN σ, AVERAGE DURATION OF STIMULI (*e*) AND TIME INTERVAL BETWEEN STIMULI (*p*), GIVING OPTIMAL MOVEMENT FOR DIFFERENT *O*s

O	*e*	*p*
N	48	173
B	56	207
W	49	176
H	54	203
G	54	203

Protocols. We next present a few protocols from the *O*s. The impression of the *O* is generally that his eyes follow the moving light across the field. This is the conviction of the more unpractised *O*s especially, and is more true for the M.O. series than for the S.O. series. This is to be expected because in the M.O. series the stimuli come at regular intervals and may therefore be anticipated in spite of attempts to overcome this tendency on the part of the *O*s. Note the following protocols and then compare with the records and our explanation of them:

W "I was aware of an eye-movement. In spite of the fact that I tried to keep my eye upon the fixation-point, I had the impression that my eyes would jump to the first spot and move across to the second. I do not believe my eyes left the fixation-spot in spite of this feeling."

N "I thought that sometimes my eyes wandered at the first of the series of five exposures but I was sure they never did during the last one or two trials." (*E.* "What kind of wandering was it?") "I felt that when the light came on I looked at it and followed it across." "In one of the M.O. series I thought I felt eye-movements between stimuli. These were distinct from the kinaesthesis or strain which has always accompanied the apparent movement, that is, my eyes seemed to move in front of that strain as a background which I always feel."

B, "Even though I tried to fixate the red light I felt some eye-movement. I thought that my eyes moved almost with the light, but they couldn't move quite fast enough."

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[5] A statement of these laws and their implications will be found in H. Helson, The psychology of *Gestalt*, this JOURNAL, 36, 1925, 509.

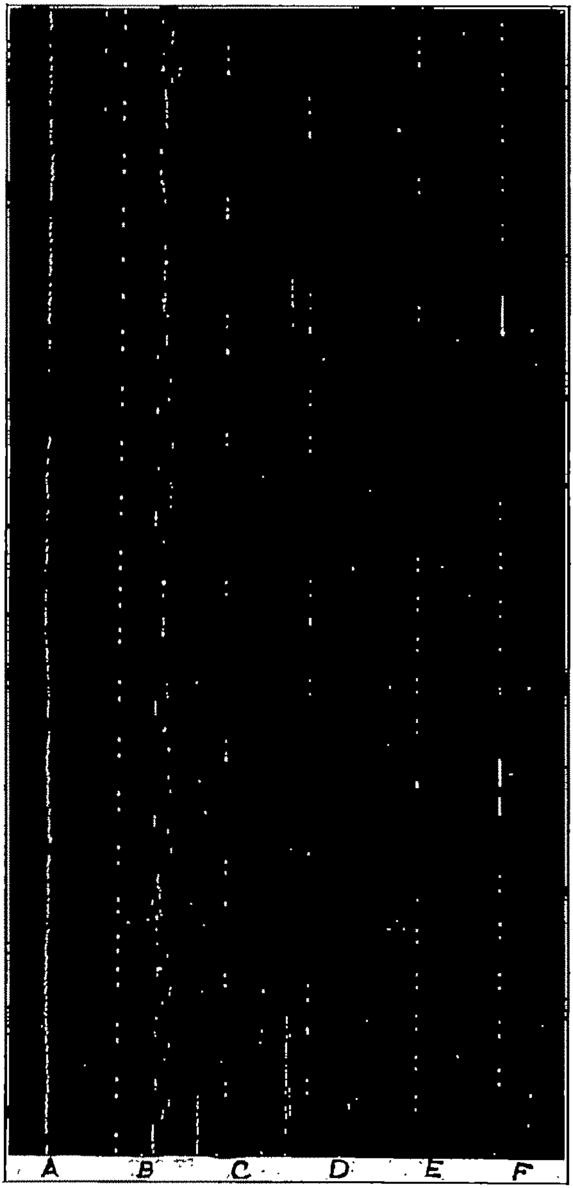

PLATE I
The records should be read from the top of the page down.
For explanation of the records see the text.

It will be seen that our *O*s report the kinaesthesis and strains which Higginson's *O*s reported. The only court of appeal, then, is the objective record of what the eyes were actually doing at the time the movement was seen, unless one resorts to the theory that the phi-phenomenon is due not to overt eye-movements but to strains and *incipient* movements. Apart from the fact that such a theory is open to many of the objections we shall urge against overt movements, it is a theory which hardly anyone would wish to defend, first, because it is not open to observation and experimental test, and secondly, because it suffers from vagueness and ambiguity in leaving the whole question in the dark as to the amount of strain or incipient movement necessary in the production of large, small, dual part and full movements. We therefore leave the question of unobservable movements and proceed to an examination of the records.

Plate I is to be read from the top down. Record A was taken without recording the occurrence of the stimuli. The periodic tremors in the line occur at the correct interval to be associated with the stimulus. The presentation was M.O. The tremors are undoubtedly *head-movements*, not eye-movements, for this record was made before a rigid biting board was provided. After securing the *O*s' heads no such records were obtained.

Record B shows just how far the eyes would have to move to satisfy Higginson's theory because here *O* attempted to follow the stimuli with voluntary eye-movements. *O* first fixated the red fixation-point, then he looked at stimulus A when it came on and finally at stimulus B when it appeared. If one compares this record with the remaining records where the *O* held his fixation as instructed, it will be noticed that while movement was seen, there is neither enough movement of the eyes nor movement in the proper directions to account for the seen movement.

Records C and D were made during S.O. series, the stimuli being so set as to give movement about half the time. Records E and F show what occurred during M.O. series.

Our conclusion is that in spite of reports of eye-movements, strain and kinaesthesis, the corresponding photographic records show no evidence whatever of eye-movements of such significance either in amount or direction to correlate with seen movement. The distinct kinaesthesis which one feels as the two lights are flashed on in succession must be merely shifts in tonicity as the stimulus-field changes. Indeed, we have as much right to assert that the perception of movement is responsible for the felt kinaesthesis as to say that kinaesthesis or strain is responsible for apparent movement!

Higginson has given nine classes of facts which he believes are explained by the eye-movement hypothesis,[6] neglecting a number of facts not only in the literature from the work of others but also facts which he himself has reported which are fatal to this hypothesis. We shall first take up the facts he advances to support the eye-movement theory, after which we shall proceed to a discussion of the other facts which, according to the canons laid down by Higginson for any satisfactory theory, also have a claim upon our attention.

(1) *Os report ocular movement.* Introspections in which kinaesthesis from the eyes are reported support this statement. Yes, eye-movements may be reported, but are they present? Do they take place at the proper time and in the proper direction and are they large enough to explain the movement *actually seen*? Our photographic records show that they do not.

(2) *Presence of intermediate members.* The time interval between the exposure of the first stimulus and the second may be made not only too short for eye-movement from one stimulus to the other, but altogether too small for several successive fixations. Furthermore, the eye is blind during eye-movement and if the eye happened to be in motion before the appearance of the first stimulus, it would be impossible not only to see movement, but even more, the intermediate members described by Higginson.

(3) *Movement of negatively colored objects.* Higginson attributes this to change in fixation which give rise to negatively colored after-images. But Higginson does not state explicitly how he conceives eye-movements (change of fixation) can be responsible for this phenomenon. If the eye shifts its fixation from A (first stimulus) to B (second stimulus) while stimulus A is still acting upon the retina, this would make stimulation A cross over some retinal elements giving rise to seen movement, not in the direction of B, but away from it. If the eye does not shift in the direction of B until after stimulus A is cut off, then as the eye shifts to B, assuredly, the projected after-image of A would move across to B. But it is rare to get a negative after-image in the time given, and after such brief stimulation, the positive after-image is the one that should appear.[7]

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[6] Higginson, *op. cit.*, 37, 1926, 408 ff.

[7] Fischer pointed out that the after-image cannot be the negative after-image because of the shortness of the time of exposure and the time interval between stimuli. In no case was he able to get a negative after-image in stroboscopic exposures. Higginson in the first article cited in footnote 3 (p. 65) wrongly interprets Fischer to mean that the negative after-image is necessary for the perception of movement in the stroboscope when he translates *Phasen* as after-image. The following paragraph from Fischer (*op. cit.*, 148) proves our point conclusively: "Es lag nun nahe, zu untersuchen, ob die Nachbilder, welche die stroboskopischen Erscheinungen vermitteln, unter Umständen complementär ausfallen können. So zahlreiche Versuche aber auch in dieser Hinsicht mit verschiedenfarbigen Punkten von mir angestellt worden sind, so habe ich doch in keinem einzigen Falle ein complementär gefärbtes Nachbild beobachten können. Der Lichteindruck ist bei allen diesen Erscheinungen so kurz, dass nur ein gleichfarbiges Nachbild entstehen kann."

Higginson does not state explicitly which of the above two interpretations is to be given to his theory. The first interpretation cannot, on purely physiological grounds, give rise to movement in the required direction, and is open to the very serious objection involving the time element as well as the resulting kind of after-image which should appear in the time available.

(4) *Movement beyond the second member.* Higginson states that the movement overshot the second stimulus and subsequently withdrew to it before disappearing, or the second stimulus was forced out of its position. In these cases the observer saw "a weak line where no line could possibly exist." This is no more mysterious than seeing apparent movement between discrete stimuli. We may answer this argument in Higginson's own words used in another connection, "one cannot argue directly from the physical order to the phenomenal." Yet Higginson demands eye-movements to save the constancy hypothesis! We too have heard *Os* exclaim at the great amount of 'overshooting' of the movement beyond the second stimulus object. This type of movement may be related either to *gamma* or *delta* movement. If *delta* movement, it can be explained on the basis of the conditions worked out for *delta* movement by Korte. Certainly at present it remains a most mysterious affair with no promise of being cleared, since, if eye-movements are not responsible, Higginson has no hypothesis whereby this fact of apparent movement can be linked with the large body of facts resting on fairly sound theoretical and experimental basis.

(5) *Dual movement.* Dual movement is cited by Higginson as further proof of his hypothesis. This type of movement is also explained by means of the bolstering hypothesis of after-images, a theory which was effectually disproved as early as 1912 by Wertheimer to whom we refer the reader.

(6) *Spatially dislocated lines.* Oblique lines may be seen as horizontal or *vice versa.* But how do eye-movements explain this finding? We are not told by Higginson. Moreover, the same phenomenon is found in apparent movement on the skin and hence we need an explanation that will carry over from one sense modality to another. We suggest that a spatially dislocated line may be due to the following factor: orientation in the dark (on the skin under other conditions) is normally very poor. It is affected (as *Os* have found in the Aubert phenomenon) by such factors as visual streaming phenomena, reported by one of the writers in his study of the autokinetic illusion, where it was found that eye-movements, on the contrary, favor good orientation or restore correct orientation which has been lost during steady fixation.[8] We should therefore regard spatial dislocation as a special case of the more general disturbances in spatial reference found in viewing a restricted field with no anchorage points or when only single or small parts of the field are illuminated, the rest being dark.

(7) *Momentary disappearance of the first member during movement of the second.* This may be a case of dual part movement where one stimulus moves, disappears, and the second takes up the movement. Or it may be due to

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[8] J. P. Guilford, Autokinesis and the streaming phenomenon, this JOURNAL, 40, 1928, 401-417.

momentary inhibition of the first by the second which appears suddenly. In all of these cases Higginson has failed to state precisely what his conditions were and to control the mental set of his *Os*.

(8) *Movement of a single line.* This must be sought in something besides eye-movement because our records show the eye is still at the time of the appearance of the first stimulus. The direction of the movement will be determined by cortical set (used in a definite sense) as pointed out by Wertheimer[9] and DeSilva.[10]

(9) *Phenomenal differences in intensity, brightness and extent of stimulus objects.* When both stimuli were given simultaneously the *Os* reported that the stimuli were equal in brightness, extent and intensity, but when presented successively one member might be apprehended as more intense or brighter than the other. If the eyes move, all is made clear, we are told, but how it is made clear we are not told. Here again, we have an example of the constancy hypothesis in extreme form, for it is assumed that lines presented simultaneously and appearing equal in various respects should appear the same when presented successively. Factors other than changes in fixation or eye-movements may very well be responsible for this state of affairs, not the least of which may be mentioned is the fact that after the exposure and apprehension of one stimulus, both retinal and central processes undergo considerable modification which is bound to influence the appearance of the second stimulus, if it is seen.

Now there are further reasons why the eye-movements theory fails to meet the demands made upon it, both from the point of view of theory and of fact, of which Higginson was aware since he has stated what we expect of a good theory and has himself reported observations which are fatal to the eye-movement hypothesis. Nor was he unaware of the checks made by Wertheimer against eye-movements, which he reported in 1912[11] and which we reproduce here.

(*1*) The total time of the seen movement (from the exposition of the first stimulus to the disappearance of the second) was less than 100σ whereas 130σ are necessary for eye-movements.

(*2*) When the fixation point was held, the seen movement was just as good as when the eyes were allowed to move over the field during the exposures.

(*3*) Check against eye-movements was made by projecting after-images in which very slight movements could be detected—they remained stationary while apparent movement was good.

(*4*) The after-image of the first stimulus remained after the exposition of the second, holding its place, showing no movement had occurred.

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[9] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-265. The six points made by Wertheimer against the eye-movement hypothesis which we reproduce below will be found on pp. 181-185 of the article cited here.

[10] H. R. DeSilva, An experimental investigation of the determinants of apparent visual movement, this JOURNAL, 37, 1926, 469.

[11] *Op. cit.*

(*5*) Movement in opposed directions simultaneously in the same field disposes also of incipient, unobservable eye-movements, as well as the hypothesis of movement of after-images.

(*6*) Real movement and the apparent movement seen in the moving picture theater cannot be satisfactorily explained by the eye-movement hypothesis.

In an article published a month earlier than the second one containing his eye-movement hypothesis,[12] Higginson had urged as an objection against the configurational theory of apparent movement that it did not explain the apprehension of movement in different directions seen simultaneously. Needless to point out this objection is even more fatal to the eye-movement hypothesis as advanced by Higginson. Moreover, in his first article on movement, Higginson described conditions under which it is possible to see apparent movement from stationary stimuli when the temporal interval between the two stimuli is zero. He tells us that with zero interval "for the first time we fail to find any report of 'no movement'. Full movement may occur under phenomenal simultaneity." In these cases Higginson would have to assume that even though his *O*s were instructed to maintain their fixation, every time the first stimulus was exposed the eyes were already in movement since no movement could be initiated in a zero interval. This assumption hardly seems plausible when we consider his statement that with zero interval *O*s perceived full movement *every time*.

We find, therefore, in studies which have appeared since the publication of Wertheimer's 1912 article, and in Higginson's own published reports, that there existed a number of observed facts and adequate checks sufficient to make the eye-movement hypothesis absolutely untenable. Higginson knows what the requirements of an acceptable theory are. He says,[13] "The final test of any scientific assumption is experimental verification. If the facts of *extended* observation (italics ours) fail at substantiation, a doctrine can have little worth." We may add that a theory which leaves the negative cases out of account is worthless. Why did Higginson fail to consider, in his presentation of his eye-movement hypothesis, facts which were in the literature before his time, facts which he himself believed to be crucial for any theory and which he urged against other theories?

In view of the large amount of work done on phi-phenomenon on the skin and in audition, the time has long since passed when a theory of apparent movement from stationary stimuli can lay claim to serious consideration which does not admit of application in more than one sense modality. It goes without saying that in the case of movement in 'bows' and arcs through the air above the skin and underneath the skin as reported originally by Benussi,[14] the assumption of something peripherally moving on the skin breaks down as badly as the peripheral explanation does in the case of visual movement. The finally acceptable theory of apparent movement will have to be, if not more complex, at least very different from any theory yet proposed, since some facts cannot be pictured according to known physical processes because nothing comparable to them appears to be known in electrical physics. Certainly our objective records which fail to reveal any correlation between eye-movements and seen movement, as well as the numerous other checks against eye-movements, make any such simple explanation as the eye-movement theory absolutely impossible as an acceptable explanation of the phi-phenomenon in its manifold variety.

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[12] G. D. Higginson, Apparent visual movement and the *Gestalt*, *J. Exper. Psychol.*, 9, 1926, pp. 228-252.

[13] *Ibid.*, p. 228.

[14] V. Benussi, Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psychol.*, 36, 1917, pp. 59-135.

SUMMARY

In this article we have reported the results of some experiments in which eye-movements were photographed during the phi-phenomenon. No correlation whatever was found between eye-movements and the occurrence of apparent movement. If anything, eye-movements interfere with good phenomenal movement from two successive stationary stimuli.

Higginson's support of the eye-movement theory was critically examined and found to be without serious justification. Some of his own facts, as well as other facts reported since Wertheimer's study of 1912, cannot be explained by an eye-movement theory and they even tend to disprove any such theory for movement in general. Any general theory of apparent movement must take account of *all* the facts, not only of visual, but also of auditory and tactual movements from stationary stimuli.

SOME CONDITIONS OF THE PERCEPTION OF VISIBLE MOVEMENT

By Forrest L. Dimmick and Robert W. Sanders, Hobart College

During the past decade few single problems in psychology have received more attention than has the visual perception of movement.[1] It has formed the systematic rallying point of one school, certain aspects of it have been analytically described, new forms of it have been pointed out, and the influences of various factors have been studied. Numerous investigations have been concerned with the subject since the appearance of Wertheimer's observations in 1912.[2] Yet the fundamental questions remain unsettled. One must choose among several hypotheses or be content with various interesting details that have little systematic bearing on the main problem. On the other hand many of the articles are experimental and deal therefore with the facts of observation, and facts do not belong to any single system of psychology. Moreover, the stimulus conditions for all the experimental studies have been similar and the phenomenal criterion, the meaning of 'movement,' has been common to all of them. It should be possible, therefore, to bring all of the results into some sort of agreement, though of course, the systematic interpretations of them will remain diverse. In this experiment we set out to examine some of the factors concerning which there has been apparent factual disagreement, namely, the length of the temporal interval and the intensity of the stimulus.

Apparatus. Our experimental setting was essentially the same as that of the earlier investigations. The apparatus consisted of two projectors which were focused through an opening in one wall of a dark room upon a white screen 3 m. distant. The openings into the dark room were exactly fitted to the objectives of the projectors so that the latter were entirely outside and no light entered except that projected through the lens. Opaque slides, each having a narrow slit, one above and one below the center, were placed in the

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*Accepted for publication March 21, 1929.

[1] Presented at the meeting of the American Psychological Association in New York, December 27-29, 1928.

[2] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-265.

slide carriers. The projected images of the slits were horizontal lines 1 cm. by 10 cm. and 10 cm. apart. The exposure times were controlled by a falling shutter placed at the nodal point of the optical system. The maximum error of this shutter was less than 5σ. The exposure time of each line was 30σ and the temporal intervals between the successive lines were respectively 0, 30, 60, and 90σ. The intensity of the stimulus was varied by interposing a ground glass at two different positions between the source of light and the slide. These gave stimulus intensities of (1) 'brilliant white,' (2) 'good white,' and (3) 'dull white.'

Observers. Our *O*s were three undergraduates majoring in psychology, J. C. Karl (K), A. Bacon (B), K. Cornwell (C), and the senior author (D). The first three *O*s had no knowledge of the problem, of the procedure, or of the apparatus of the experiment. While their general training in psychology included an understanding of the technical distinctions between 'process' and 'meaning,' the *O*s were not required to make this precise in their reports. They distinguished, however, almost invariably between what they 'actually saw' and what the experience 'seemed like,' 'reminded them of,' or 'meant.'

Instructions. The *O*s read through the following typewritten instructions at the beginning of every observation. "A stimulus will be shown you which will arouse a visual perception. Describe the perception as completely and as accurately as you can. Report no process of which you are not sure. The stimulus will be repeated to complete the description." The *O*s supplemented their verbal reports with sketches in which they indicated the spatial-qualitative pattern of the perception. Five series were presented with every combination of time interval and intensity. Prior to the regular series every *O* was given from 10 to 20 practice series until his reports became reasonably stable.

RESULTS

Temporal interval. The reports of our *O*s fall under the principal rubrics worked out in the earlier investigation, viz., simultaneity, succession, and movement. The latter was either complete or partial, *i.e.* filled the whole field or only part of it.

Table I gives the percentage of reports by every *O* of the several perceptual experiences with every combination of time interval and intensity. Table II shows the frequency of the perceptions at the different times irrespective of the intensity of the stimulus. In the above tables the partial movement forms appear relatively infrequently and then mainly at the shorter intervals. On the other hand, complete or optimal movement is almost totally absent from the reports of the three principal *O*s when the temporal interval is 0σ. Those of D who worked with knowledge of the procedure and who therefore may have been predisposed toward the perception show 24% optimal movement reports in *one* set of observations. The great majority of reports at this

TABLE I

SHOWING THE PERCENTAGE OF THE VARIOUS PERCEPTIONS EXPERIENCED BY EVERY *O* WITH EVERY COMBINATION OF TIME INTERVAL AND INTENSITY

(Un = unimembral movement; Bi = bimembral movement; Op = optimal movement; Su = succession; Si = simultaneity)

Temporal interval		B					C					K					D				
		Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
0	1	0	4	0	0	96	0	4	12	0	84	4	0	8	0	88	4	8	24	0	64
	2	0	8	0	0	92	8	8	12	0	72	0	0	4	12	84	0	0	0	0	100
	3	0	8	8	0	84	16	0	0	0	84	16	0	4	20	60	27	0	3	0	70
30	1	0	0	4	4	92	8	0	20	48	24	4	8	24	52	12	28	12	44	16	0
	2	0	20	12	20	48	4	0	56	40	0	0	0	48	32	20	20	0	60	20	0
	3	4	0	36	60	0	0	0	80	12	8	0	0	40	28	32	7	0	80	13	0
60	1	0	0	60	40	0	0	4	52	44	0	0	0	80	20	0	12	8	64	16	0
	2	4	0	52	44	0	4	0	76	20	0	2	0	56	32	0	0	20	60	20	0
	3	0	0	48	52	0	0	0	100	0	0	0	0	52	48	0	0	0	100	0	0
90	1	0	0	32	68	0	0	0	76	24	0	0	0	84	16	0	20	12	20	48	0
	2	0	0	40	60	0	0	0	36	56	8	4	0	92	4	0	0	20	40	40	0
	3	0	0	56	44	0	0	0	24	64	12	0	0	72	28	0	20	20	0	60	0

TABLE II

SHOWING THE EFFECT OF TEMPORAL INTERVAL UPON THE REPORTS

(For an explanation of the abbreviations see Table I)

Temporal interval	B					C					K					D				
	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
0	0	7	3	0	90	8	4	8	0	80	7	0	5	11	77	9	4	20	0	67
30	1	7	17	28	47	4	0	52	33	11	1	3	38	37	21	20	7	58	15	0
60	1	0	54	45	0	1	1	77	21	0	4	0	63	33	0	8	8	69	15	0
90	0	0	43	57	0	0	0	45	48	7	1	0	83	16	0	17	15	17	51	0

time interval are of simultaneity. When the temporal interval is increased to 30σ, optimal movement appears relatively frequently, simultaneity decreases, and succession appears. With further increase of the interval simultaneity disappears and optimal movement increases. At the 90σ interval, succession increases at the expense of optimal movement.

These results do not of themselves settle the question of the dependence of optimal movement on the length of the temporal interval, but they do add evidence to one side. Wertheimer reports that the shorter intervals give simultaneity,[3] the longer ones, succession, and the intermediate ones movement. Our earlier studies agree with these findings and give a quantitative confirmation of this relation between the three perceptions and the temporal interval. Higginson,[4] on the other hand, reports the "apprehension of movement" of the complete type in some 85% of his presentations, irrespective of the temporal interval. An interval of 0σ is as efficacious as any other. It should be possible, now, to duplicate this later set of results with our *O*s, but first we must clear up certain differences of interpretation. So far as we can judge, the phenomenal aspect of the perception with which we are dealing is the same as Wertheimer's. That is, the movement is visible, or seen, even though his *O*s (or ours) report no visible object which moves. Our *O*s sometimes make the explicit statement that movement is "meant" when it is not "seen," but such cases are ordinarily excluded from their reports by the instruction: "Report no process of which you are not sure." As an example:

> B, [90σ, intensity 1]: "A line appeared near the center of fixation then it moved down—No! it didn't move!—I couldn't *see* the movement but another line appeared below and the first disappeared."

As over against this, Higginson embraces under his heading, "full movement," every apprehension of movement that is referred to the whole field whether or not it is seen in that area. This difference of interpretation undoubtedly accounts for some degree of the preponderance of "full movement" in his results.

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[3] *Op. cit.*, 178.

[4] G. D. Higginson, The visual apprehension of movement under successive retinal excitations, this JOURNAL, 37, 1926, 63-115.

There remain, however, the large number of simultaneities and successions in Wertheimer's results and in ours that are absent from Higginson's. These observations are prior to the interpretations and demand further experimental examination. With this in mind, we altered our procedure. The crucial point, it seemed to us, might lie in the *Os'* attitude. Consequently, we altered our instructions to read, "You will be shown a stimulus which may or may not arouse a perception of movement. State whether you perceive movement. If you do, characterize the movement in every way that you can." No change whatever was made in the stimulus conditions. Table III shows the effect of the predisposition to perceive movement upon the *Os'* reports. The percentage of perceptions of movement for all *Os* is more than doubled. We are justified in concluding, therefore, that some factor operated in the observations of Higginson's *Os* to precisely the same end as the attitude set up by our second instructions. If, then, we are concerned with the phenomenon of *visible* movement, which is conditioned primarily by a set of stimulus conditions and not by an attitude of the *Os*, the temporal interval is an essential factor for the determination of the perception. On the other hand a predisposition to perceive movement will nullify the effect of any single stimulus factor or group of factors, and the meaning, "movement of a visual object," or just "visual movement," may be carried by a context which may include any sort whatever of sensory datum or it may be implicit in the situation and taken for granted. The last two forms of the perception do not come within our understanding of the term 'visible movement.' For the sake of labeling them we may call them "apprehension of movement from visual stimuli" and "inference of movement."

TABLE III

SHOWING THE EFFECT OF PREDISPOSITION UPON THE REPORTS
(For an explanation of the abbreviations see Table I)

Time interval	Observers														
	B					C					K				
	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
0	0	0	100	0	0	0	0	87	0	13	0	0	47	0	53
30	0	0	100	0	0	13	0	87	0	0	0	0	73	0	27
60	7	0	93	0	0	0	0	100	0	0	0	0	93	7	0
90	0	0	100	0	0	0	0	87	13	0	0	0	67	33	0

Intensity. The second factor upon whose influence we hoped to throw light is intensity. We took Korte's "intensity" to mean intensity of light, and we tried to approximate his conditions in our projection apparatus by having three intensities of the projecting light.[5]

TABLE IV

SHOWING THE EFFECT OF INTENSITY OF STIMULUS UPON THE REPORTS
(For an explanation of the abbreviations see Table I)

	Observers									
Intensity	B					C				
	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
1	0	1	24	28	47	2	2	40	29	27
2	1	7	26	31	35	4	2	45	29	20
3	0	3	37	39	21	4	0	51	19	26

	Observers									
Intensity	K					D				
	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
1	2	2	49	22	25	16	10	38	20	16
2	4	0	50	20	26	5	10	40	20	25
3	4	0	42	31	23	12	5	50	18	15

Tables I and IV give indication of some influence of the intensity of the stimulus on the perceptual experience. Table IV shows the percentage of the various perceptions as they depend upon intensity irrespective of the temporal interval. From this table we should be forced to conclude that intensity is not a factor in the perception. If we turn back to Table I, however, we find that at a given temporal interval the frequency of optimal movement varies with the intensity of the stimulus, though not unequivocally. In the main, with temporal intervals of 30σ and 60σ optimal movement increases with increase of intensity. At 0σ and 90σ intervals changes of intensity produce little or no change in the perception. Thus we cannot confirm Korte's work in detail, but neither do we dispute it. The condition, we believe, is not as simple as he makes it.

The existential correlate of visible movement. So far we have been dealing with the phenomenal aspects of movement, and our results are, in the main, in accord with the results of the Gestalt school. We were concerned, further, with a more adequate description of the sensory carrier of 'visible movement,' which we have reported previously as a "flash of gray." The process-meaning relation of this 'gray flash' to visible movement, is as definite for the present set of *O*s as for Dimmick's *O*s at Cornell,[6] and Dimmick and Scahill's *O*s at Michigan.[7] The observational task is far from easy, so that a complete description must be built up from successive presentations. The following examples chosen from the reports of the several *O*s give characteristic observations of the qualitative aspects of the perception.

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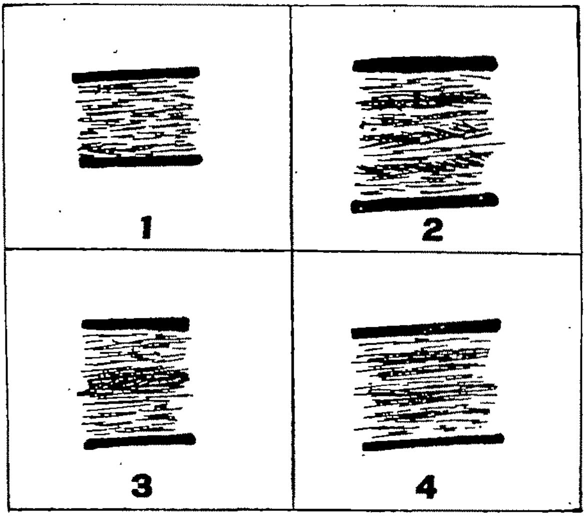
[5] A. Korte, Kinematoskopische Untersuchungen, *Zsch. f. Psychol.*, 72, 1915, 193-296.



FIGS. 1–4. DIAGRAMS ILLUSTRATING THE DIFFERENT KINDS OF OPTIMAL MOVEMENT

SAMPLES OF REPORTS

Optimal movement. B, [60σ, int. 1, Fig. 1]: "A line appeared near the center of fixation. It changed position so quickly that it gave the effect of a series of parallel horizontal lines which, however, were indistinct and blurred together, and much grayer than the line itself. I could see the movement down to the lower position where the line appeared distinctly with a patch of gray above it."

C, [60σ, int. 2, Fig. 2]: "I saw a white line in two positions. It started at the top and moved to the bottom very rapidly giving the effect of hazy gray lines in between."

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[6] F. L. Dimmick, An experimental study of visual movement and the phi phenomenon, this JOURNAL, 31, 1921, 317-332.

[7] F. L. Dimmick and H. G. Scahill, Visual perception of movement, *ibid.*, 36, 1925, 412-417.

K, [90σ, int. 2, Fig 3]: "I saw three lines. The middle one was less distinct than the first and last. Between were blotches of light gray. The impression I got was that a line moved from top to bottom."

D, [60σ, int. 1, Fig. 4]: "A line appeared in the first position with the field below it gray and lighter near the line. Then the second position came with the gray patch still above it. The perception was that of a line moving rapidly from one place to another. There were streaks in the gray which looked like the line in movement."

Simultaneity. B, [0σ, int. 1]: "There were two parallel white lines near the center of the fixation field which came and went together."

C, [0σ, int. 1]: "Two parallel lines appeared rapidly and disappeared. They were simultaneous."

K, [0σ, int. 1]: "I saw two parallel light lines for a very short time."

D, [0σ, int. 1]: "Two horizontal lines came simultaneously. They flashed on and off and were stationary."

Succession. B, [90σ, int. 1]: "A line appeared in the middle of the fixation field. It disappeared and another appeared below its position and parallel to it. They were not together but in discrete succession."

C, [90σ, int. 3]: "That time there were two distinct lines; first one then the other in succession. There was nothing in the background."

K, [90σ, int. 3]: "I saw two quick flashes of light, one beneath the other and following it."

D, [90σ, int. 1]: "There were two successive lines. The first came and went and then the second did likewise."

There seems to be no doubt that the movement which these *O*s saw is qualitatively gray, tinged perhaps with the color of the stimulus lines. When the color of the stimuli was either red or green, the carrier of 'movement' remained predominantly gray. Sometimes it was slightly reddish or greenish but as frequently just gray. When one stimulus was red and the other green, gray predominated though it was sometimes tinged with one of the colors.

This 'gray' which is the fundamental existential correlate of visible movement has the following characteristics. (1) It is never a surface but varies from a true film toward a bulky color. The *O*s describe it sometimes as a "curtain" of velvety gray "without a definite surface." At other times it lies in three dimensions though the exact distances can not be stated. (2) Its localization is indeterminate. It lies somewhere near the plane of the stimulus line but it is never confined to it. The *O*s are never sure whether it lies before or behind that plane. Sometimes, as we noted above, it extends into space behind. (3) It is "unsteady," "flickering,"[8] "light and dark in various parts but not in a fixed pattern." This instability shows no direction of change. (4) The area is a constant factor in any position, *i.e.* the size of the gray 'patch' does not change. (5) The temporal aspect is always noted as very brief.

As we have previously shown, the 'visible movement' set up by two discrete, stationary stimuli is perceptually indistinguishable from that resulting from a moving stimulus,[9] and their descriptive aspects are identical. In the case of the latter, *i.e.* the moving stimulus, the qualitative filling of the movement field may be ascribed to a positive after-image of the stimulus. This would be gray when the stimulus and background are black and white, and a low chroma of the stimulus color when the latter is colored. The source of the gray with the stationary stimulus is not so obvious. We may ascribe it to the ordinary tied image. Such, indeed, seems to be the explanation for the coloring which sometimes appears. We suggest, however, that the gray which forms the qualitative matrix of 'visible movement' is another instance in which 'central gray' is projected and becomes visible in the visual field. Its appearance and the conditions of its occurrence are comparable with those of Jaensch's "*Zwischenmedium*,"[10] and Möller's "glassy" perception.[11]

Our conclusions are in essential accord with the findings of another investigator who has attacked the problem from a very different angle. DeSilva examined phenomenologically the effect of a number of factors on the perception.[12] His conclusion that "the perception of movement is a complex, variable experience" gives substantiation to our tentative classification into 'visible movement,' 'apprehended movement,' and 'inferred movement.' The common term 'movement' does not guarantee that the experiential factors or the phenomena are the same. Higginson's,

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[8] It is significant to note here that, whereas we describe movement as "flickering grey," the author of a recent phenomenological study of flicker concludes that flicker is essentially movement. Cf. Kurt Haack, *Experimental-deskriptive Psychologie der Bewegungen, Konfigurationen und Farben unter Verwendung des. Flimmerphänomens*, 1927, 25 ff.

[9] F. L. Dimmick and H. G. Scahill, *op. cit.*, this JOURNAL, 36, 1925, 412-417.

[10] E. Jaensch, Ueber die Wahrnehmung des Raumes, *Zsch. f. Psychol.*, Erg. Bd. 6, 1911, 244-307.

[11] E. F. Möller, The glassy sensation, this JOURNAL, 36, 1925, 249-285.

[12] See footnote, p. 616.

DeSilva's, and our own results indicate that they are not. DeSilva points out in several places that an 'object consciousness' or 'meaning attitude' enhances the perception, that is, insures the presence and definiteness of the meaning 'movement.' Conversely, an analytic attitude or repetition of the stimulus dissipates the 'movement.' In addition the effects of background, fixation, and circumscription upon the *visibility* of the movement which DeSilva determined are what one would expect if, as we suggested, "central gray" is the basis of the 'gray flash' which carries the meaning 'visible movement.'

Summary

(1) From our results and their relation to those of other investigators in the field we contend that the conceptual term 'visual movement' is not adequately definitive either of a single mental phenomenon or of a single perceptual pattern. We suggest tentatively, the rubrics 'visible movement,' 'apprehended movement,' and 'inferred movement.'

(2) The basic sensory correlate of 'visible movement,' which may be identical with the phi-phenomenon, is an unsteady or flickering bulky-film of gray which in the case of the stroboscopic stimulus, consists primarily of projected "central gray" modified in a particular case by the qualities of the stimuli and of the background and by perceptual images of the order of 'memory color' or 'tied images.'

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[12] H. R. DeSilva, An experimental investigation of the determinants of apparent visual movement, this Journal, 37, 1926, 469-501; Kinematographic movement of parallel lines, *J. Gen. Psychol.*, 1, 1928, 550-577. See especially page 554, footnote 6, where DeSilva calls attention to the fact that in our last study of visual movement we discontinued the use of separate 'meaning' instructions. This methological change does not imply a confusion of the two types of report on the part of the *Os* after they had acquired adequate practice in observation. This practice was attained by sheer repetition of the stimulus. Reports at the beginning were confused and wholly inadequate on the descriptive side (see DeSilva). The *Os* were trying, as *Os* always do, to evade the descriptive task by finding out what the experimenter "wanted them to say." Failing in this, sooner or later all of them described the experience itself. They always made interpretative statements, however, about the descriptions they had given. This obviated the necessity of our taking a duplicate set of reports of meaning. Our first group of trained *Os*, on the other hand, did not give interpretations under the instruction to 'describe.' The similarity of the descriptive reports under such great differences of psychological sophistication argue against any artificiality of the existential data. In addition, DeSilva's *Os* are in essential agreement with our own.

AN EXPERIMENT ON AFFECTIVE CONTRAST[*]

By ALBERT J. HARRIS, Harvard University

The law of affective contrast is, to the best of my knowledge, supported by the results of but one experiment, that published by Bacon, Rood, and Washburn in 1914.[1]

In this experiment a large number of *O*s made absolute judgments on the pleasantness or unpleasantness of colored papers, on a scale from 1 to 7. For some of the *O*s, a set of 18 colors was presented, first with each preceded by one of six previously chosen pleasant colors, then with each preceded by one of six unpleasant colors. For others the unpleasant colors were presented first and the pleasant colors afterwards. For *O*s working without knowledge, "every observer, who had the colors presented first after unpleasant and later after pleasant colors, had more affective values lowered than raised by the process; twenty-three out of twenty-five to whom the colors were shown first after pleasant and later after unpleasant colors had more affective judgments raised than lowered on the second presentation."[2]

The authors think that these results certainly demonstrate the influence of contrast, and formulate the law of contrast as follows: "The pleasure of an agreeable experience is heightened if it is preceded by a disagreeable experience, and an impression in itself unpleasant may be felt as pleasant if a more unpleasant state has been its antecedent. In like manner unpleasantness may be heightened or even created through contrast with a preceding agreeable affective state."[3] These results conform equally well, however, to a law recently proposed by Dr. J. G. Beebe-Center and termed the law of affective equilibrium. The law states that "the affective value of the experiential correlate of a stimulus

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[*] Accepted for publication May 18, 1929.

[1] M. M. Bacon, E. A. Rood, and M. F. Washburn, A study of affective contrast, this JOURNAL, 25, 1914, 290-293.

[2] *Op. cit.*, 292.

[3] *Op. cit.*, 290 f. The statement follows that these are laws "which everyday experience has established;" however, no other definition of affective contrast is given in the paper.

varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group."[4]

The question now arises as to which of these two laws is the more adequate to describe the phenomena usually referred to as affective contrast. The essential difference between the two laws is that the former makes the change in the affective value of a stimulus a function of the conscious event immediately preceding the application of the stimulus, whereas the latter makes it a function of all the conscious events preceding the application which constitute with it a unitary group. In order to determine which of these laws is the more adequate, it would seem to be sufficient to find out whether, in an experiment involving a contrast effect, the different judgments upon a single stimulus vary in a definite way with the affective value of the immediately preceding experience. The experiment described below is designed to answer this question.

TECHNIQUE

The stimuli used were 90 Milton-Bradley colored papers, mounted on four-inch squares of white pasteboard. The *O*s were Messrs. K. W. Oberlin, W. D. Turner, and H. L. Kim, and Miss D. Selling, all research students in the Harvard labortory. They will be designated hereafter in this paper by the first two letters of their surnames.

The 90 cards were spread out on a table, and each *O* was asked to select the 10 most pleasant and 10 most unpleasant colors. This gave a P (pleasant) determining series and an U (unpleasant) determining series for each *O*. From the 41 colors that were left untouched by all four *O*s a representative series of 20 was selected by *E*. It was called the test series, and was the same for all *O*s.

A haphazard order of presentation was arranged, with determining and test colors alternated, so that every determining stimulus was followed once by each of the test stimuli. This order was kept throughout the experiment. The colors were presented serially against a background of medium gray. The exposure was 4 sec., and the interval between colors was 5 sec. The colors were approximately level with the eyes of the *O*, and about 4 ft. from him.

The instructions were as follows: "Colors will be presented to you one at a time. You are to make a report of their pleasantness or unpleasantness, judging them as +3 (very pleasant), +2 (pleasant), +1 (slightly pleasant), 0 (indifferent), −1 (slightly unpleasant), −2 (unpleasant), or −3 (very unpleasant). Judge each color independently, and avoid all memory or association effects."

The experiment proper consisted of two parts. For *Ob* and *Tu*, Part I consisted of judgments on the intermingled P series and test series, and Part

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[4] J. G. Beebe-Center, The law of affective equilibrium, *ibid.*, 41, 1929, 64.

II of judgments on the intermingled U series and test series. For *Se* and *Ki* the U determining series was given in Part I and the P series in Part II. Each part (consisting of 400 judgments) was completed in three experimental sessions, one week apart. The first and third sessions in each part consisted of 120 judgments, and the second of 160 judgments. A rest of 5 min. was given after every 40 judgments. There was an interval of one week between the choosing of the determining series and the first trial of Part I, and an interval of three weeks between the last trial of Part I and the first trial of Part II.

Possible Sources of Error

One possible source of error of considerable magnitude is the attitude of *O* Bacon, Rood, and Washburn reported that *Os* with knowledge of the purpose of the experiment gave contradictory results. *E* tried to keep the *Os* ignorant of the purpose of the experiment, and of the nature of their results. Introspective reports obtained at the conclusion of the experiment showed that he was successful with all four *Os*.

Another possible source of error is the influence of memory of the preceding judgments. The *Os* were instructed to try to avoid this effect. In their introspective reports they agree that memory of a previous judgment on a color occurred in relatively few cases. When a memory did occur, *O* was able most of the time to make his judgment independently of the memory. It seems to the writer that the influence of memory on the results may therefore be regarded as negligible.

A third—and most serious—possible source of error is the arithmetic treatment of the judgments in the calculation of the results. To the writer this method seems the best available. Bacon, Rood, and Washburn found which way the majority of the shifts (obtained by presentation after pleasant and unpleasant stimuli) occurred in most of their *Os*. Their procedure is open to two objections. In the first place, the determining stimuli are not constant. The results of the present experiment show, on the one hand, that the pleasant determiners are not equally pleasant, nor are the unpleasant determiners all equally unpleasant; and, on the other hand, that a determiner usually called pleasant (or unpleasant) may occasionally be judged to be the opposite. A valid method of treatment must take this variability into account. In the second place, there is a question which concerns the size of the shifts involved. It seems to the writer that a shift from 'unpleasant' to 'very pleasant' must be treated as greater than a shift from, say, 'indifferent' to 'slightly pleasant.' The only way to take account of such differences is to use an arithmetic scale. Its use does not mean that the arithmetic values are taken to mean equidistant, constant points on a fixed scale. They are, in such a case as this, conventions for the convenient expression of similarities and differences that cannot otherwise be stated and compared except in a very awkward and roundabout way. The writer further believes that the use of these conventions brings out general tendencies in the results more clearly and accurately than any other method.

Results

(*1*) *Individual contrast.* The results from each *O* were treated separately. From the judgments made on the determiners the average affective value of each determiner was found, and the determiners were arranged in rank order of pleasantness. Every test stimulus was presented once after each of the determiners, so the average affective value of the test stimuli following a determining stimulus could be made the basis for comparing the effect of various determiners on the affective value of the test stimuli. The results for each *O* in Part I are shown in Fig. 1. If contrast is an individual effect, in the sense of depending solely on the immediately preceding experience, the average value of the test colors following the least pleasant determiners, those on the left, should be higher than the average value following the most pleasant determiners, those on the right. The results show no such tendency. The figures of the corresponding results in Part II are shown in Table I. It was felt to be unnecessary to give both figure and table for each part.

TABLE I

Showing the Average Value of the Test Colors Following the Different Determiners

The determining colors are arranged in rank order of pleasantness, with the least pleasant at the left. Braces indicate that two or more determiners had the same affective value.

O	Determiners									
	1	2	3	4	5	6	7	8	9	10
Tu	.45	.0	.85	.15	.80	1.10	1.10	−.45	.55	.60
Ob	−.25	−.05	.15	.15	.45	−.05	.25	−.35	.20	.50
Ki	.75	.25	.65	.65	.85	.75	.50	.80	.40	1.10
Se	.25	.10	.25	.0	.40	.20	.35	.0	−.05	−.15

Not too much reliance should be placed on these calculations, however, for they are based on the *average* value of the determiners. If the value of each determiner were constant, this method would be valid, but, since variability always occurs when colors are used as affective stimuli, the use of the average value of the determiners is unsafe. To avoid this difficulty another method of calculation was devised.

A table was constructed in which the value assigned at each presentation of a test color was shown under the value given to the determiner which immediately preceded it. Two such tables were made for each *O*, one for the part with the U determining series, and one for the part with the P determining series. Table II shows a representative portion of one of these tables. The ideal procedure would be, of course, to compare the average value of the test colors following −3 determiners with those following +3 determiners. However, there were few +3 judgments given to P determiners, and few −3 judgments to U determiners. The average of a column with few figures, such as the −2, +2, or +3 columns in Table II, has little value, so it was necessary to select columns for comparison. In each of the eight tables the two

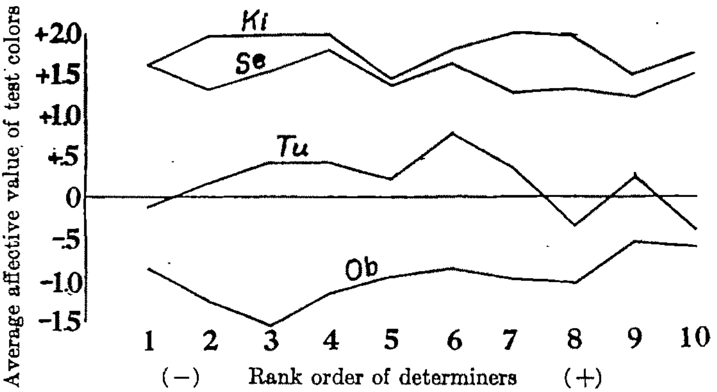

FIG. 1. AVERAGE VALUE OF THE TEST COLORS FOLLOWING THE DIFFERENT DETERMINERS

If there were individual contrast, the lines would tend downward from left to right.

TABLE II

PART OF ONE OF THE TABLES SHOWING THE VALUE GIVEN TO EACH TEST COLOR UNDER THE VALUE GIVEN TO THE IMMEDIATELY PRECEDING DETERMINER AT EACH PRESENTATION

OBSERVER—*Se* DETERMINING SERIES—U

Test	Determiner values						
colors	−3	−2	−1	0	+1	+2	+3
1	+1, +3		+3, +3, +3	+3, +3	+3, +3, +3		
2	0, +1		−1, 0, +2, 0	−2, +1	+1, +1		
3	−2		0, +1, +1, +1	+2, +1	+1, +1, −1		
4	+2	+2, +2	+2, +3	+2, +3	+2	+3	+1
5	+2, +1		+1, +1	+1, +1	+2, +2, +2	0	
etc.							

columns were selected which were farthest apart, and which at the same time included values for nearly all of the test colors. Thus in Table II the −3 and +1 columns were selected. Eighteen of the 20 test colors were represented in both of these columns. The other two were disregarded, and the average value of the 18 test colors in these two columns was taken; in this case, it was +1.7 in the +1 column, and +1.5 in the −3 column. The averages of the selected columns from each of the eight tables are shown in Table III. If there is an individual contrast effect, the right-hand figure of each pair of averages in this table should be lower than the left-hand figure.

TABLE III

Showing the Average Values of Test Colors Following P and U Determiners, in Both Parts for All *O*s

(See text)

O	Determining series	Determiner values							Contrast
		−3	−2	−1	0	+1	+2	+3	
Tu	U		+.55				.28		yes
Ob	U		−.07			.13			no
Ki	U			.20				−.04	yes
Se	U	1.26				1.43			no
Tu	P		−.05					−.25	yes
Ob	P			−1.35				−.50	no
Ki	P					.79		1.05	no
Se	P					.20		−.04	yes

It is apparent from the table that, with a given *O* and a given set of determiners, the value of the test stimuli does not vary in any definite relation to the value of the preceding determiner. In fact, one *O* shows a slight tendency toward a contrast effect with both P and U determiners, one *O* shows the reverse in both cases, and the other two *O*s show contrast once and the opposite of contrast once.

TABLE IV

Showing the Difference in the Average Value of the Test Colors When the Determining Colors are Changed

O	Test average after P determining series	Test average after U determining series
Tu	0.15	0.50
Ob	−0.97	0.02
Ki	0.69	1.73
Se	0.01	1.42

This method of calculation fully confirms the results of the first method. In both cases the results show small deviations from a neutral state, and the distribution of these deviations is of the type to be expected of pure chance variations. It seems safe, therefore, to conclude that there is, in this experiment, no evidence of an individual contrast effect, *i.e.* of one depending solely on the immediately preceding experience.

(2) *Mass contrast.* An inspection of Table IV shows that, for three *O*s, the average value of the test stimuli is very markedly

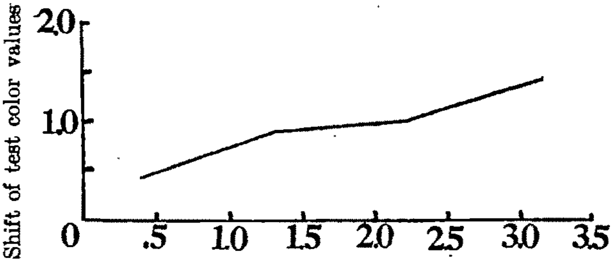

FIG. 2. SHOWING HOW THE AMOUNT OF SHIFT IN THE AFFECTIVE VALUES OF THE TEST COLORS VARIES IN RELATION TO THE DIFFERENCE BETWEEN THE TWO DETERMINING SERIES

higher with U determiners than with P determiners. In the case of the fourth *O*, (*Tu*) the same tendency is present, but it is not as marked as in the other three cases. The difference between the two determining series, as measured by the difference between the average affective values of the two series, was not, however, the same for the different *O*s. It seems natural that a large difference between the two determining series should bring about a greater shift in the test values than a small difference. An inspection of Fig. 2 shows that this is just what occurred. For each *O* the amount of shift in the test values is plotted vertically, and the difference between the two determining series horizontally. Not only is the shift in the test series larger the greater the difference between the determining series, but the four points yield a practically straight line. Approximately, the amount of shift in the test values varies proportionally with the difference between the determining series.

CONCLUSIONS

We have seen that there is no demonstrable individual affective contrast effect. On the other hand, the affective value of a series of stimuli is changed when another group of stimuli is interpolated. The value of the test series is higher when an unpleasant series of stimuli is interpolated than it is when a pleasant series is interpolated, and the amount of shift is proportional to the difference between the determining series. This might be called a "mass contrast" effect. It is adequately described by Beebe-Center's law of affective equilibrium: "The affective value of the experiential correlate of a stimulus varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group."

The question as to just what stimuli can constitute a unitary group remains as yet unsolved. It may be that an equilibrium effect is possible with stimuli of different modalities. The writer hopes that experiments which he is now making will shed some light on this problem.

VARIATIONS TOWARD PURPLE IN THE VISUAL AFTER-IMAGE

By THEODORE KARWOSKI, Harvard University

The correct appreciation of the phenomena of visual after-images has suffered from over-simplification. In general, psychologists believe in the existence of a negative after-image of exactly complementary hue, and in the existence of a positive after-image; and they know something of the standard conditions under which each of these occurrences can be induced. The belief in these simple, clearly defined phenomena has come about because the well-known theories of color have tended to fix norms and to provide no explanation for phenomenal deviations from them. The elementary text-books have also, undoubtedly for pedagogical reasons, ignored deviations from these two standard types of after-images, and then the belief, once established, has reënforced itself by tending to limit research to those conditions under which the accepted results are usually found. Nevertheless, the experts have always known that the phenomena of after-images are neither as fixed as the theories require nor as definite as the text-books declare. Helmholtz[1] recognized the existence of variations. Parsons[2] gives warning of the existence of variations. So do Troland[3] and McDougall.[4] The phenomena need to be studied with the more careful control of conditions that spectral light yields and under extremes of intensity as well as in the middle ranges.

The present study is designed to accomplish a survey of the phenomena of after-image at a variety of intensities of the stimulus and of the field of projection. It is confined to the use of monochromatic light because impure lights introduce unpredictable variations and lessen the precision of the results.

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*Accepted for publication April 30, 1929.

[1] H. von Helmholtz, *Physiological Optics*, Amer. ed., 1924, II, 261.

[2] J. H. Parsons, *Introduction to the Study of Colour Vision*, 1915, 102.

[3] L. T. Troland, Preliminary note: The influence of changes of illumination upon after-images, this JOURNAL, 28, 1917, 497.

[4] W. McDougall, Some new observations in support of Thomas Young's theory of light and color vision (III), *Mind*, N. S. 10, 1901, 17–18.

APPARATUS AND PROCEDURE

The apparatus consisted of two units—a spectral light system and a white light mechanism. The light from both units entered the eye after traversing the same pathway. Along this course a diaphragm, shaped like a half-moon, eliminated part of the beams, so that the light was seen as a semi-circular image. This diaphragm was at a distance such that the image fell within the foveal area. The eye was equipped with an artificial pupil, 2 mm. square, which adequately controlled any changes that might have been caused by the pupillary action.

Fig. 1 illustrates the optical system employed. In it S is the spectral light source; W is the white light source.[5] The light beams from W are reflected from the magnesium coated disk, M, along the same course taken by the rays from S to the eye. C.T. is the tube of a spectrometer with its slit from which the divergent rays come. These rays are collected by the lens, L, and are changed into parallel beams. M.G. is a magnifying lens; P is an artificial pupil through which observations are made.

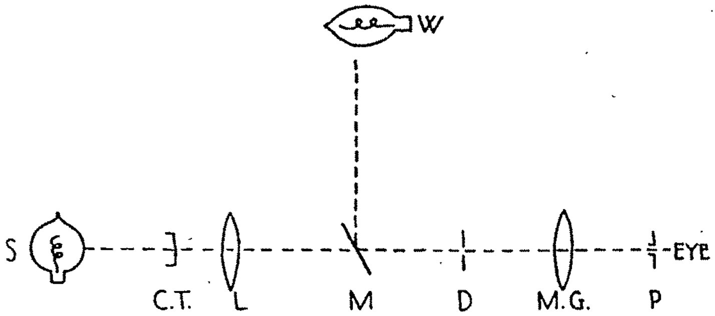

FIG. I. AFTER-IMAGE APPARATUS

The experimental procedure was as follows. The magnesium disk could be moved out of the path of the beam from S by pulling a string which revolved the disk, M, into a position such that it was no longer in the path of the rays from S . After the eye had been fatigued with a spectral color, the disk was suddenly and noiselessly brought into the path of S, thus cutting off its radiations. The after-image immediately followed, projected on the magnesium disk as a field.

The apparatus contained supplementary features by which it was possible to measure photometrically the stimuli studied and also the lights used as reacting fields. Since the white light would vary in color with the incandescence of the lamp, Wratten Photometric Filters were used in such combinations so that all the white reacting fields matched Cambridge noon daylight.

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[5] The light sources employed were as follows: S is Mazda (G.E.) specially gas filled lamp, 6 volts, 4 amperes; W is a Street Series lamp, 6.6 amperes, S24, with Skirted Edison Base, vertical coil tungsten filament, Type C lamp.

These filters were numbers 78, 78A, 78B and 78C. The matching with daylight was accomplished by means of a special mechanism devised for this particular purpose.

In this investigation all the spectral lights, except violet, were studied at 592, 148, 37 and 9.2 photons. The eye was exposed to the stimulus for 24 sec. and the after-image was projected upon white reacting fields of 592, 148, 37, 9.2, 4 and 0 photons. The *O* was required to note particularly the first phase of the after-image. Thus a constancy in attention was assured. Other after-images were observed but they were not recorded unless they were sharply distinguished from the first phase. The spectral lights used as stimuli were: 650, 580, 528, 458 and 428mμ.

RESULTS

The significant results pertain to hue only. In respect to the other attributes of visual sensation, it may be said, on the basis of these experiments, that in general the brightness of the after-image depends directly upon the brightness of the field upon which it is projected. There evidently is a mixture between the brightnesses in the two cases. With increased brightness of the reacting field there is desaturation of the after-image. The most saturated after-sensations occur with medium values of the background.

The data for hue are shown in the tables. Tables I–V are for spectral lights of 650, 580, 528, 458 and 428mμ respectively. Each column gives results for a brightness, expressed in photons, of the spectral light for a given series. The rows of the tables show the brightness levels of the reacting fields. The tables, therefore, read as follows. Column 1 of Table I shows, for spectral light of 650mμ at 592 photons, the results of projecting the after-image of this light on the various backgrounds of 592, 148, 37, 9.2, 4 and 0 photons.

TABLE I

SHOWING RESULTS WHEN STIMULUS OF 650 mμ WAS USED

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592	148	37	9.2
592	B Bg	G Gb	Gb	*
148	B Bg	Gb	Gb	
37	Bg B Bg	Gb B G	Gb G	Bg
9.2	Bv B Bg	Bg B	G Gb B	Bg B
4	Bv B	Bg B Bg	Gb	Bg B
0	V	Bg	Gb	Bg

*On bright reacting fields with after-images of weak lights there is merely a slight discoloration of the background. I am reluctant to call this a typical after-image.

Column 2 gives similar results for the same color, at the brilliance of 148 photons. When the after-image is a succession of colors, a series of color-symbols is given as each item of the table. Such a succession was always gradual or involved a blending of one color into another, for there were no colorless gaps. The letters in the tables stand for the following colors: R = red, O = orange, Y = yellow, G = green, B = blue, V = violet, and P = purple. These letters are often qualified by small letters which stand for colors of less dominance in the blend than the color indicated by the large letters. Thus Bg means a blue-green in which blue is dominant.

TABLE II

Showing Results when Stimulus of 580 mμ was Used

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592	148	37	9.2
592	Pb	B	Bg	
148	P	B	Gb	
37	Pr Pb	Pb B	Gb G	G
9.2	Pr B P	Pb Pr	Bg	G
4	Pr B P	Pb B P	B	Gb G
0	Pr	Pb	V	B

TABLE III

Showing Results when Stimulus of 528 mμ was Used

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592	148	37	9.2
592	Pr	Pr R	Pr	
148	Pr R	Pr O	Pr O	
37	Pr	Pr	Pr	Or
9.2	Pb P	Pr	Pr	OR Oy
4	Vb V	Pb	Pr	Or R Pr
0	Vb	Vb	Pr	Pr

TABLE IV

Showing Results when Stimulus of 458 mμ was Used

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592	148	37	9.2
592	O Yg Y	Or O Yg	Oy	
148	O Yg Y	Or O Yg	Oy O	
37	R O Y	O Yg	Y Yg	Yg
9.2	Pr O Y	Or Yg	O Yg	Yg
4	Pr O Y	Or Yg	O Y	Yg
0	P Yg	Pr Yg	Yg	Yg

TABLE V
SHOWING RESULTS WHEN STIMULUS OF 428 mμ WAS USED

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592*	148	37	9.2
592		Oy Yg		
148		Oy	Yg	
37		O Gy Yg	Y Yg	Gy
9.2		Or Yg	Yg	G
4		Pr Oy Yg	Gy Yg	G Gy
0		Pr Pr	Gy	G

*The blank column at brightness level 592 photons, is due to the fact that violet of this brightness could not be obtained by the apparatus.

In order to understand the tables, the reader must have well in mind the complementary colors of the spectral lights investigated. The complementary for 650mμ is blue-green or 490mμ; for 580mμ, blue or 475mμ; for 528, purple; for 458, yellow or 572mμ; and for 428mμ, yellow-green or 567mμ.

Frequency of the complementary hue.

650mμ. The blue-green complementary always forms one phase of the after-image in all instances.[6]

580mμ. The blue complementary is represented in all instances except at the lowest intensity. With very weak stimulation the complementary color is not experienced.

528mμ. The purple complementary is always represented.

458mμ. The complementary hue is less pronounced at 9.2 photons than are the orange and yellows of higher intensities.

428mμ. The complementary yellow-green is usually found but at the higher intensities it occurs only as one phase in the after-image.

Hue and intensity of reacting fields.

650mμ. Hue changes are not very pronounced except at the brightness levels of 4 and 0 photons. At these points the blue component is usually strengthened, particularly when the stimulus is intense.

580mμ. At 592 and 148 photons the red component of the purple is enhanced with decreasing values of the white backgrounds. In case of 37 and 9.2 photons the blue component, in contrast to green, tends to be enhanced for lower intensities.

528mμ. The hues tend to become more bluish with decreasing brightnesses of the reacting fields except in the case of 37 photons. The more characteristic fact is that the red-purple is the invariable hue.

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[6] The exact complementary does not seem to be experienced in after-image and contrast colors. Colors that produce white have been tested by the after-image and contrast methods and the complementary color is either too blue or too red in comparison with the complementary of the white light equation. Cf. H. D. Cook and F. M. Kunkle. The qualitative relation between complementary and contrast colors, *Psychol. Monog.*, (no. 96), 1916.

458mμ. At 592 photons the red component is enhanced at the lower intensities. At 148 and 37 photons there is but slight change and none at 9.2 photons.

428mμ. At 148 photons there is a change from orange to purple or from red to blue; at 37 photons, from yellow to green.

Hue and intensity of the stimulus.

650mμ. Comparing the columns in Table I it is at once apparent that at 592 photons after-images are quite bluish. All of the after-images are either blue-green, blue, or violet. At 148 photons the blue component is weaker but still quite pronounced, at 37 the green is dominant, and at 9.2 photons there is a reversal toward blue.

580mμ. At 592 photons the appearance of the red component is a striking fact. At 148 photons the red component becomes weaker and the blue is enhanced. At 37 photons the green component is brought out and is still further enhanced at 9.2 photons.

528mμ. There is less variation with intensity than in the other colors. At 592, 148, and 37 photons the hue series are essentially in terms of the complementary purple. At 9.2 photons, and to some extent at 37 photons, the red component is enhanced.

458mμ. The red component is strong at 592 photons, persists at 148, is very much weakened at 37 and is eliminated at 9.2 photons.

428mμ. At higher intensities the red and blue components are both present. They disappear at 37 photons, where yellow and green are pronounced. At 9.2 photons the yellow is weakened.

Conditions most favorable for the complementary after-image.

650mμ. At 37 photons.
580mμ. Between 148 and 37 photons.
528mμ. The complementary color is invariably experienced under all conditions except low brightnesses of the reacting field.
458mμ. At 37 photons.
428mμ. At 37 photons.

Strikingly uncomplementary after-images.

650mμ. The blue and violet at higher intensities.
580mμ. The purplish effects at higher intensities.
528mμ. Nothing unusual; the purple is here complementary.
458mμ. The purple after-images at 592 photons.
428mμ. The purple after-images at 148 photons.

These results show that the complementary color was experienced under some conditions for all the colors studied. It is, however, more specifically determined for some intensities of the stimulus than for others. It is most pronounced at the lower intensities. Here the after-images are confined in the limits of variation or succession of hues. These variations approximate the complementary color. At higher intensities the characteristic after-image is in terms of red or blue, usually both.

The most significant results are the changes of hue that were found to be a function of the intensity of the stimulus. They may be summarized as follows.

650mμ. Blue or violet to green-*blue* to blue-*green* to green-*blue*.
580mμ. (Purple) Red to blue to blue-green to green.
528mμ. (Purple) Blue to red.
458mμ. (Purple) Blue to red to yellow to yellow-green.
428mμ. Red and blue to green-*yellow* to yellow-*green*.

The changes of hue as functions of the reacting field are less significant than for intensity of the stimulus. There is very little change except at the lowest photon values of 4 and 0. From 592 to 9.2 photons the changes are essentially in terms of components already present. It would seem that above 4 photons the reacting fields act as essentially white backgrounds; below 4 photons as dark backgrounds. Complementary colors are favored at the upper region and deviations toward red or blue are introduced at the lower level. The suggestion of Miles to distinguish 'after-images' proper,[7] which are solely the result of the continued activity of the visual tracts, from the 'projection after-image,' which is in large measure influenced by reflected light from the area upon which it is projected, is in keeping with these results.

The intensity of the stimulus and the background may be related in terms of a combination of factors. When the stimulus is intense and the reacting field is weak, the purplish colored after-images are favored; when the stimulus is of medium intensity and medium brightness, the complementary after-image is facilitated.

DISCUSSION

The first question that arises is as to whether the hue of the stimulus changed at the various intensities so as to produce the different after-images that have been noted. The so-called Bezold-Brücke phenomenon describes the changes of the hue of a spectral light as a consequence of reduced intensity of the light radiations. The law is illustrated by Exner[8] as follows: "If the color tones in the spectrum arise in general from simultaneous but unequal excitation of the three fundamental sensations, such a color tone will alter when the objective intensity is reduced, and will approximate more and more closely the hue of the strongest of the fundamental sensations composing it, since the weaker ones will sink below the threshold. One can verify this deduction by gradually reducing the brightness of the spectrum. The transitions between red and green, and green and blue, and the violet all vanish, leaving only the three fundamentals or sensations approximating."

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[7] G. H. Miles, The formation of projected visual images by intermittent retinal stimulation, *Brit. J. Psychol.*, 7, 1915, 420–434; 8, 1915, 93–126.

[8] F. Exner, Ueber die Grundempfindungen im Young-Helmholtzchen Farbensystem, *Wiener Ber.*, III (2a), 1902, 857.

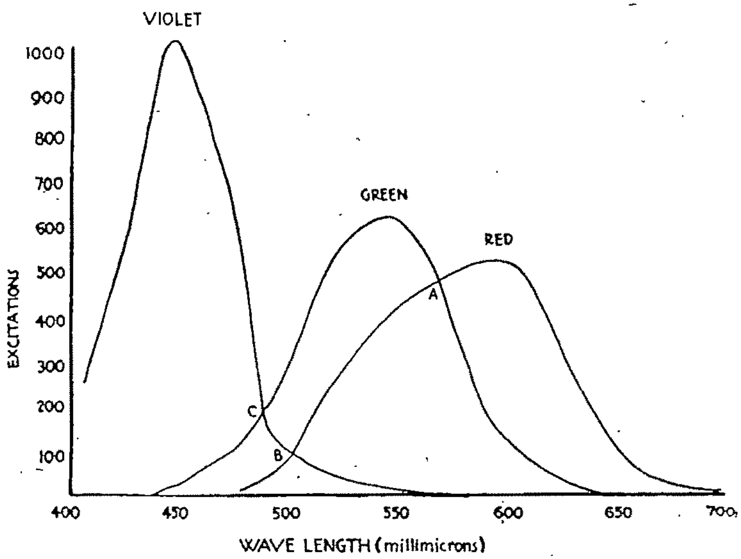

Fig. 2—Excitation Curves

The application of this principle to the excitation curves of Fig. 2 is as follows: "'A' equals R and G—no B. The hue will be independent of brightness within wide limits; not so wave-lengths on either side of 'A'. A wave-length to the redwards of 'A' will become redder when the brightness is diminished, since the G will sink under the threshold. One to the greenwards of 'A' will become greener. 'B' equals G plus (equal red and blue). The hue will be independent of brightness. The wave-lengths on either side will be shifted in hue toward the yellow and blue respectively."

'C' is likewise an invariable and wave-lengths on either side of it will vary toward blue and green respectively.[9]

These curves do not explain the purplish after-images at high intensities. However, by making an assumption that is consistent with visual theory, the explanation can be considerably improved.

Suppose that we start from the low intensities and interpret the law in the reverse direction, since we are interested in explaining those after-effects of bright lights that are apparently incongruous with visual theory. Except at the invariable points, the dominant hue would be alone active at the very lowest intensities. With increasing intensities subordinate excitation-processes would tend also to increase. Suppose now that each excitation-process has a maximum or limit of functions, such that when all fundamental sensations are functioning maximally they combine to give white.[10] As soon as the dominant excitation-process has reached its maximum, further increase would tend to raise the subordinate excitation-processes while the dominant sensation remains relatively stable. Such a change would continue, gradually, until all the excitation-processes are functioning maximally and white would finally be experienced. One fact in support of this hypothesis is that all colors eventually become white if sufficiently bright.

Interpreting the observed results in accordance with these suppositions, we find that the blue-green at the lowest values of 650mμ is the complementary after-image which is typical for this stimulus. At a higher intensity the after-image is more bluish, a fact which means that the green excitation-process is being added to the red, causing it to be more yellowish. At the very highest intensity, the green process is still more pronounced and the red is more yellowish; and thus we can account for the richer blues and violets which were noted. In this case the blue excitation-process is not involved at all, as Fig. 2 shows.

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[9] Exner actually used the curves of König and Dieterici in this description, but I have substituted in its place the excitation curves as determined by Weaver who averaged the data of König and Dieterici with those of Abney. The argument is equally true for those curves as for König and Dieterici's. Report of the Colorimetry Committee of the Optical Society of America, 1920–21, *J. Opt. Soc. Amer.*, 6, 1922, 548.

[10] H. von Helmholtz, *op. cit.*, II, 183.

Spectral yellow at 580mμ is not quite at the 'invariable point' but is placed rather toward the red. The red sensation is dominant and at low intensities this sensation is almost pure. Hence, we have the green after-image at low intensities. With higher luminosities, the green excitation-process is enhanced and the red and green sensation acts as if the eye were stimulated with an addition of yellow in the red, due to the mixture of the green and red processes. The blue-green after-image would thus be explained. At the highest intensities the red and green sensation would act as a yellow and the blue component in the after-image would be pronounced. This conclusion, therefore, falls short of accounting for the red component in the purple experienced at these levels. It seems that a greater addition of a green sensation is necessary.[11]

In the case of the green, we may explain the dominance of the red component at low intensities as due to the dominant green sensation. At the higher intensities the blue component would be increased with the addition of yellow caused by the mixture of red and green. Hence this accounts for the fact that the purples are more bluish at higher than at lower intensities.

Spectral light 458mμ may be similarly explained. At low intensities the complementary color or yellow-green is due to the relative purity of the violet excitation-process. At higher intensities the green process is more effective and tends to add red to the after-image. This red would be quite pronounced at high luminosities.

Spectral light 428mμ would behave similarly to 458mμ but the difficulty here is that the green excitation-process does not extend as far as this point.

This theory explains the results only in part. It explains some of the purplish effects at high intensities, and also the fact that for long wave-length of light the blue component is enhanced, while for the short it is the red component. This explanation is not, however, complete, for the purple is not adequately explained in the case of stimulus 580mμ nor the purple for 428mμ, where the violet should be an invariable color (according to the diagram of excitation curves) because no other excitation-processes are involved. A similar situation exists at 650mμ, where apparently a pure red sensation yields after-images that indicate that it is quite mixed.

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[11] It is possible, however, to regard the purple, in this case, as a fusion of the after-images of the red and green excitation-processes. Ordinarily, it would be expected that the after-images are determined by the objective stimulus mixtures and that the red and green should determine after-images of a bluish kind.

Much of the difficulty could be remedied if the green excitation curve were more extensive at the base, and the fact that purple results from the stimulating of yellow at high intensities indicates that probably these curves overlap much more than is apparent from the different descriptions of the excitation curves. The theoretical curves as originally formulated by Helmholtz overlap extensively and so more readily explain the results of this experiment than do the empirically determined curves.

One other matter that we have to mention is the fact that the after-image changes in hue at very low intensities of the reacting field. This finding may be explained, in part, by the fact that hues change with the admixture of black to them. Rood[12] showed that hues change when colored papers are mixed with black. Mayer[13] investigated contrast effects by observing 15 shades of gray on different colored papers. "Viewing in succession the gray disks on a violet surface, no. 1, 2, 3, and 4 (grays) appear yellow-green, no. 5 appears as much yellow as green, no. 6 ap-appears yellowish-green. The following disks in the order of their appearance appear greener and greener till no. 13 appears of a dark green almost devoid of yellow." Thus, it would seem that in this case the light gray backgrounds enhance the yellow component, while the dark gray papers enhance the green component in the contrast color. The same fact is also apparent in the after-images observed with stimulus 428mμ, and, in general, it may prove to be the explanation of change of hue at the lowest intensities.

CONCLUSION

The color of the after-image is specifically correlated with the brightness of the light simultaneously fatiguing the retina. Thus the after-image is complementary in hue only under particular circumstances. It is no more characteristic of the stimulus than is the purplish effect which is due to the same light under different intensities. A complementary hue is favored by medium intensity of the stimulus and reacting field; a purple hue is a consequence of the higher intensities. The curves of the excitation-processes indicatet hat the complementary effects are due to the dominant process, whereas the changes introduced by increased intensity of the stimulus may be due to the increased activity of the subordinate excitation process. However, the Bezold-Brücke phenomenon, which describes the hue of spectral lights as a result of change in their brightness, does not fully explain the hue of the after-image.

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[12] O. N. Rood, *Modern Chromatics*, 1897, 197–201.

[13] A. M. Mayer, Note on the analysis of contrast color—by viewing through a reflecting tube a series of gray discs or rings on colored paper, *Amer. J. Sci.*, 4th ser., 1, 1895, 38–40.

THE INFLUENCE OF REPETITION OF A SERIES UPON THE OMISSION OF ITS INTERMEDIATE TERMS

By Albert D. Freiberg, Karl M. Dallenbach, and Edward L. Thorndike

From time to time some student of behavior advocates or assumes the doctrine that the frequent occurrence of a series of physiological or psychological events (call it A→B→C→D→E) will tend to cause the omission of one or more of the intermediate terms so that in the end A→B→C→D→E tends to be replaced by A→E. Thus Jennings writes: "When a certain physiological state has been resolved, through the continued action of an external agent, or otherwise, into a second physiological state, this resolution becomes easier, so that in course of time it takes place quickly and spontaneously."[1] "The law may be expressed briefly as follows: The resolution of one physiological state into another becomes easier and more rapid after it has taken place a number of times. Hence the behavior primarily characteristic for the second state comes to follow immediately upon the first state. The operations of this law are, of course, seen on a vast scale in higher organisms in the phenomena which we commonly call memory, association, habit formation and learning."[2] This law may be expressed conveniently as a tendency of a series of states $A \rightarrow B \rightarrow C \rightarrow D$ to become $A \rightarrow D$ or $A \rightarrow B' \rightarrow C' \rightarrow D$. B' and C' being states B and C passed rapidly and in a modified way so that they do not result in a reaction but are resolved directly into D.

Hollingworth writes, in his explanation of how false responses are eliminated and the correct response is selected, "with repetition . . . these intermediate acts drop out."[3] Hollingworth seems to attach a certain potency to the firstness of the first member of the series as well as to the repetition of the series. The end member, according to him, tends to be produced by that one of the preceding members "which first or unfailingly occurs."[4]

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*Accepted for publication July 14, 1929.

[1] H. S. Jennings, *Behavior of the Lower Organisms*, 1906, 289.

[2] *Op. cit.*, 291.

[3] H. L. Hollingworth, *Psychology: Its Facts and Principles*, 1928, 214.

[4] *Op. cit.*, 216.

"The one which first and always occurs (hunger pang plus sight of food) will at once instigate the consequent (pulling the catch)."[5]

Thorndike has elsewhere discussed this doctrine in general.[6] The present note concerns detailed evidence bearing on it found by the examination of the mistakes made by the compositors of 1068 galleys of *The American Journal of Psychology*, comprising the years 1923-1927 inclusive. The action of a compositor in setting the type for a word, whether by hand, monotype machine, or linotype machine, is a fairly typical case of such a series of events. The sight of the word in the copy or the thought of it in memory is the first term or *A*, the movements of taking and setting the type from the proper compartments or pressing the proper keys are the *B*, *C*, *D*, etc., the last term of the series being the setting of the last letter.[7]

If frequency of occurrence of such a series tends to shorten the series by the omission of terms between the sight or thought of the word and the setting of its last letter, omissions of letters (1) should be much more common than additions, (2) should occur much oftener for intermediate letters than for final letters, and (3) should—other things being equal—be more common in words often set than in words rarely set.

The compositor would, for example, have strong tendencies to make *te* for *the*, and *tht* or *tat* for *that*. These would have to be counteracted by other forces; and if the compositor's control broke down at all so that he made mistakes of any sort other than those of ignorance, these tendencies should appear.

In the material which we have examined, there are 373 cases where a word has a letter or letters omitted, and 324 cases where a word has a letter repeated or a letter or letters added. The omissions do not come in intermediate positions more often than chance would prescribe, but very much less often. 16 of them are at the beginning of a word; 158 of them are at the end of a word; 209 of them are in intermediate positions. The average length of the words for which letters were omitted is approximately 9, so occurrences at the end of a word are, per opportunity, about 5 times as frequent as in intermediate positions. The omissions of intermediate letters do not occur very often in words which have been very frequently used. On the contrary, as the complete list below for 1923 and 1924 shows, they are more frequent per opportunity in rare words.

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[5] *Op. cit.*, 214.

[6] E. L. Thorndike, *Animal Intelligence*, 1911, 266-272.

[7] Sometimes, of course, the series is only a part of a word; at other times it includes more than one word.

Words from which intermediate letters were omitted. Gedach(t)nis; tur(c)ica; deficienc(i)es; intense(l)y; sugge(s)tions; as(s)umptions; non-substan(t)ial; i(rr)educible; gro(s)grain; pos(i)tions; cond(i)tioned; suc(c)eeded; fulfil(l)ing; material(i)zations; qualit(at)ive; occur(r)ing; Marse(i)llaise; quanti(ta)tive; decide(d)ly; direct(ed)ness; indiffer(e)ntly; hyg(i)enic; sla(c)ken; occur(r)ence; withdraw(a)l; psy(c)hological; decide(d)ly; equal(iz)ing; zusammenges(e)tzten; critici(s)ms; Ges(t)alt; gen(e)tic; invol(v)ed; sy(s)tem; nervo(u)s; fulfil(l)s; i(n)asmuch; moral(i)ty; conscien(c)e; criti(ci)zed; abstra(c)tions; mor(t)ality; actu(a)rial; diffi(c)ult; implicit(l)y; explicit(l)y; indescri(b)able; intermitten(t)ly; embarras(s)ment; vari(a)ble; adapt(a)tion; len(g)ths; intermitti(e)render; co(n)genital; thre(s)holds; de l'(e)space; si(m)ilar; stre(n)gthen; jud(g)mental; W(a)hrnehmungswelt; Romant(i)ker; Yuj(e)ro; on(l)y; perserver(a)ting; Gestal(t)en; l(i)kely; caut(i)ous; differ(e)ntiating; for(e)see; Charlott(e)sville; rhy(th)mische; Mü(n)chen; b(a)sis; b(r)ought; visu(a)lization; psychi(c)al; Scho(e)nheinz; neces(s)arily; skel(e)ton; c(l)ose; pren(a)tal; appetit(i)ve; pleas(a)ntness; des(s)ert; p(i)ece; definite(l)y; l(i)ght; p(l)easantness; p(l)ane.

If we take the five hundred commonest words in English reading matter—plus also the plurals of the nouns included—which compositors of five years experience would have set from a thousand or so to fifty thousand or more times, we find the following.

Omissions at the beginning of a word	5
Omissions at the end of a word	34
Omissions in intermediate positions	16
Additions	36

On the whole, the facts give no support to the doctrine that the frequent repetition of a series of events in the mind or body in and of itself produces a tendency toward the omission of the intermediate terms of the series.

THE DETERMINANTS OF CONFIDENCE

By Arthur Jersild, Columbia University

The present study is addressed to the following questions. To what extent is confidence a correlate of real skill? To what extent may a display of self-assurance be regarded as a character trait relatively independent of actual competence? What is the relation between confidence and correctness?

The experiments here reported were conducted in connection with class-room examinations. True-false tests, covering reading assignments and lectures, were administered. The plan was to obtain (1) a measure of the *Ss'* actual information; and (2) a measure of the degree of certainty or confidence which they attached to their answers.

The *Ss* were told to mark every proposition either + or − (meaning true or false). The number of propositions correctly marked as true or false constituted, accordingly, their 'Information Score.' They were further directed to indicate the degree of confidence which they felt in their judgments by using double marks (as + + or − −) when they were absolutely certain of their answers, by using single marks (as + or −) when they were only fairly certain, and by querying their marks (as +? or −?) when they were guessing. The *Ss* were led to believe that these confidence markings would be used in the computation of their grades, and that the usual formula for correcting true-false tests (number right minus the number wrong) would be applied.

For the purpose of this experiment values of 3, 2, and 1 were assigned to the three respective degrees of confidence. The total count of all these values on a given paper constituted the 'Confidence Score.' This score was computed irrespective of signs and correctness.

The above procedure was followed with two classes;[1] one of these, numbering 53 students, consisted of sophomores and upperclassmen; the other, with a membership of 32, enrolled graduate and undergraduate students. Two tests were given to each group.

Results. The method of correlation applied to the results gave the findings indicated in Table I. This table gives the coefficients of correlation between the measures of Information and Confidence. A and B designate the two respective tests given to each of the two groups. The P.E.s are given in parentheses.[2]

The first two entries in the table above show a positive correlation bteween confidence and information. It appears that actual knowledge is one determinant of a confident attitude. But the relatively low magnitude of the coefficients indicates that in individual cases high confidence may go with low performance, and vice versa.

Of greater interest is the fourth entry in the table, which shows the correlation between confidence as displayed on the two test occasions. The coeffi-

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[1] These classes were made available for this experiment through the kindness of Professor H. L. Hollingworth.

[2] The Product-Moments correlation method was used with Group 1; the Rank-Difference method with Group 2.

TABLE I

Factors correlated	Test	Group 1 (n = 53)	Group 2 (n = 32)
Grade and Confidence	A	0.44 (.08)	0.09 (.13)
Grade and Confidence	B	0.39 (.08)	0.31 (.11)
Grade	A and B	0.62 (.06)	0.58 (.08)
Confidence	A and B	0.75 (.04)	0.79 (.05)

cients shown for the two groups, 0.75 and 0.79 respectively, are decidedly higher than the values indicating the correlation between information and confidence, on the one hand, and between information scores obtained on the two tests. Of the two factors here involved, confidence and achievement, the former is the more constant. It appears that a show of confidence is to a large degree a matter of personal idiosyncrasy and that it cannot be taken as a reliable gauge of the actual merit of the performer.[3]

There remains the further question as to the relationship between confidence and correctness when the judgments of the group as a whole are taken into account. The data of the class of 53 members were examined with a view to answering the question. A count was made (1) of the total number of entries shown for each of the three degrees of confidence; and (2) of the number of errors shown in each of these categories. From this could be calculated the percentage of correct answers accompanying each of the three degrees of confidence. This procedure gave results as follows.

Degree of confidence	*Percentage of correct answers*
Absolutely certain	89
Fairly certain	71
Guess	61

These determinations show that a report of 'absolute certainty' falls a good deal short of indicating perfect accuracy; and that a 'guess' has more than a 50% chance of being correct.[4]

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[3] In an extended study, W. C. Trow (The psychology of confidence, *Arch. Psychol.*, 1923, no. 67, 1-47) found practically a zero correlation between the total achievement and confidence scores on a wide variety of tests ($r = -0.03$). He found also that the range of every *S*'s display of confidence in different situations was so great that no single test would, in his opinion, serve as an adequate indicator of a general confidence trait. In view of Trow's results, the findings in the present study, namely, that there is a reliable positive correlation between confidence and achievement, and that the *S*'s confidence will be a more constant factor than his achievement in going from one performance to another within the same type of test situation should not be given too general an interpretation.

[4] The relationship between confidence and correctness has been studied under a variety of conditions by previous investigators. See H. L. Hollingworth, Experimental studies in judgment, *Arch. Psychol.*, (no. 29), 1913, 1-119; V. A. C. Henmon, The relation of the time of a judgment to its accuracy, *Psychol. Rev.*, 18, 1911, 186-201; and Trow, *op. cit.* In each of these studies four degrees of confidence were indicated, so the results are not directly comparable with the above. The figure shown above for the highest degree of confidence (absolute certainty) closely resembles, however, the results reported by Henmon and Trow, though it is somewhat lower than the determinations found by Hollingworth. There is a close agreement between the various reports on the percentage of correctness found in the lowest degree of confidence.

Of some incidental interest in this connection is the question as to whether greater confidence attaches to an affirmation (judgment that a proposition is true) than to a denial (judgment that a proposition is false). Examination of the data revealed no statistically reliable weighting of confidence in either direction. Similarly, it might be asked whether greater accuracy attaches to an affirmation than to a denial. This problem was met by finding the percentage of correctness for all the true and all the false judgments. The results showed that 86% of all the propositions marked "+" were correctly marked; while 83% of those marked "−" were correctly marked. There is a difference of 3% in favor of the affirmations. This difference is too slight to warrant extended comment. The direction of the difference would indicate that there is a greater tendency to mark as false statements which are actually true than to mark as true statements which are false. In other words, there is greater accuracy in rejecting false propositions than in evaluating true ones. Evidence of a more distinctive order on this tendency was found by Strong in a study with advertising materials;[5] it appeared from his results that "the ability to know we have *not* seen an object is much more strongly fixed than the ability to pick out what we have seen."[6]

The main findings of the present study have been indicated above. It appears first of all that actual information determines confidence to some degree. There is a low but positive correlation between confidence and information scores and in the group results the higher the degree of confidence reported, the higher is the percentage of accuracy. But it appears also that, so far as the individual is concerned, the display of confidence is determined to a larger extent by individual caprice than by demonstrable quality of workmanship. There is a greater correspondence between the confidence shown in each of two situations than between the merit of the performance in the two instances, and in the separate situations the correlation between confidence and correctness will be low.

This latter result is amply verified by common observation. It is not unusual to encounter a person who displays utter self-assurance without showing commensurate skill, and the opposed type of individual whose show of confidence is relatively low while his demonstrated competence is high. Two factors, one may suppose, are responsible for the display of confidence which appears independently of real achievement. On the one hand, the individual may assume a confident attitude as a 'front' to compensate his recognized inadequacy. On the other hand, and this account seems more probable, the incompetence which an individual reveals in the poor quality of his performance may be associated with a corresponding lack of insight into his own deficiencies. This same principle may be urged to account for the phenomenon of high achievement associated with a relatively low display of confidence. The person of superior attainments will be able to recognize more fully that his grasp on things, however adequate to the occasion, still falls far short of thorough versatility. But the proof of this tenuous hypothesis must await further investigation.

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[5] E. K. Strong, The effect of length of series upon recognition memory, *Psychol. Rev.*, 19, 1912, 447-462. Quotation from page 462.

[6] *Ibid.*, 462 (italics mine).

AN ANOMALY OF FOVEAL COLOR PERCEPTION

By Ellis Freeman, University of Louisville

It is customary in dealing with visual perception to adopt the anatomical-physiological attitude and to suppose in general that the visual experience corresponds to the retinal excitation. Particularly in the study of visual acuity has it been assumed, without the suspicion of other possibilities, that if in two cases the ultimate retinal images be identical, the resulting experiences, that is the resolution of the complex (or, as it is sometimes called, the perception of form) will be identical regardless of the size and distance of the stimuli. This means that, under identical illumination, a large distant figure would be resolved as well as a similar small and correspondingly near figure. More broadly speaking, the view that has represented physiology, and to a large degree psychology, has been that visual acuity is accounted for by the laws of geometrical optics, that intensity of illumination and contrast and visual angle alone determined the experience of resolution of a figure or letter. In papers already published it has been shown that both foveal and peripheral acuity deviate from the inferences of geometrical optics to an astonishing degree,[1] when large distant figures and small near figures are employed, although both give identical conditions of retinal stimulation. Experiments of this nature, in addition to supplying new data, have considerable theoretical importance inasmuch as they prove the anatomical-physiological viewpoint inadequate, and show the inferences of geometrical optics, which declare that where there are identical retinal images there are identical visual perceptions, not to be universally applicable.

The present article is part of a study of the relation between visual stimuli and visual experience. Our problem here is specifically to discover whether the same anomalies occur in color perception as in the perception of form and in visual acuity. In the papers mentioned above the anomalies associated with visual acuity have been fully described. It is necessary, however, in order to show what problem these anomalies suggest for color vision, to mention them again.

Jaensch,[2] in an experiment on visual perception, obtained the following results. When the letter E was used in three sizes corresponding to the proportions 1:1.57:2.45, they were not recognized at distances which varied in the same proportion but at distances whose ratios were 1:1.123:1.496. The larger letters required a greater visual angle for resolution than the smaller. This

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*Accepted for publication August 6, 1929.

[1] Ellis Freeman, What does a test of visual acuity measure? *Arch. Ophth.*, 2, 1929, 48-56; The anomalies of peripheral visual acuity, *J. Exper. Psychol.*, 12, 1929, 324-340.

[2] E. R. Jaensch, Zur Analyse der Gesichtswahrnehmungen, *Zsch. f. Psychol., Erg. Bd.*, 4, 1909, 60-67.

result is a direct contradiction of the laws of geometrical optics. Jacobsson,[3] employing a more precise method that involved a tachistoscopic exposure of letters and geometrical figures which varied in size and distance (which was proportionate to their size), secured results similar to those obtained by Jaensch. The larger stimulus-objects were recognized correctly less often than the smaller. Von Lempicka,[4] working on the problem of color fusion, obtained similar results. She used, as her stimulus-objects, disks of four different sizes which were cross-marked with black and white bands. The width of the cross bands varied proportionally with the areas of the disks. The disks were at first placed at distances too great for her *Os* to distinguish the bands. Then the *Os* approached the disks until they reached the point at which the bands could be clearly distinguished. The results show that the large disks suffer a handicap in that they must be brought for resolution to a larger visual angle than that required by the small.

The work of Guillery[5] on the smallest retinal image necessary for the perception of color supplies a transition from the investigations concerned only with acuity to the subject of this paper. Just as other writers supposed that they were giving a constant value for acuity,[6] to which only a coefficient for distance had to be applied, so Guillery supposed that he had determined a physical constant for color sensitivity in terms of required area of retinal stimulation. His results are that at a distance of 1 m. from the eye the following diameters of the retina must be stimulated in order to produce color perception: blue, 0.019 mm.; green, 0.011 mm.; yellow, 0.007 mm.; red, 0.007 mm. The noteworthy fact is that in giving these values, Guillery thought that he was giving constants that in principle were final. Nor did it occur to him that merely with a change in distance the area of retinal stimulation might also have to be changed before the color perception could be produced.

The problem as it shaped itself for our work was, then, if a given square of color be barely perceived at a given distance, at what distance will a larger square of the same color, under the same illumination, be perceived? Will the larger be recognized at a distance proportional to its size in accordance with the laws of geometrical optics; or will it be recognized at a lesser distance creating a situation similar to that of the work mentioned here on acuity? Or, as is conceivable, will the larger be perceived at a distance even greater than one proportional to its size?

The experimental procedure was as follows: a piece of colored paper 5 mm. sq., pasted at the center of a gray cardboard 60 mm. sq., was brought toward the *O* until he just barely recognized its color. This was repeated 4 times and the average distance of recognition was computed for the 4 trials. Then the procedure was repeated with color-areas and exposure-cards that were enlarged

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[3] Malte Jacobsson, Über die Erkennbarkeit optischer Figuren bei gleichem Netzhautbild und verschiedener scheinbarer Grösse, *Zsch. f. Psychol.*, 77, 1917, 1-91.

[4] Wanda von Lempicka, Räumliche Farbenmischung auf der Netzhaut, *Zsch. f. Sinnesphysiol.*, 50, 1919, 217-251.

[5] —— Guillery, Vergleichende Untersuchungen über Raum-, Licht- und Farbensinn in Zentrum und Peripherie der Netzhaut, *Zsch. f. Psychol.*, 12, 1896, 243-274; especially 267-270.

[6] G. E. de Schweinitz, *Diseases of the Eye*, 1924, 34-36, 68-69; J. H. Parsons, *Diseases of the Eye*, 1927, 127-136.

10 diameters, *i.e.* the colored papers were 5 cm. sq. and the cardboards were 60 cm. sq. The experiments were conducted under an even gray sky on six different days with 10 *O*s. In some of the experiments the large cards were exposed first, in others the small cards were exposed first, consequently the effects of practice were equalized. The colors used were red, blue, green, yellow, and orange.

Theoretically, if one were to base his reasoning on geometrical optics as do the physiologists and ophthalmologists in discussing acuity, it would be necessary to infer that, as the intensity of illumination, the color quality, and the background gray were constant between any one small square and its corresponding large square, the only difference would lie in the fact that the large was perceived 10 times further away than the small, since it was 10 times larger. This notion is based on the generally unquestioned conviction that where all things else are equal the only determinant in question is the visual angle—the size of the retinal image—regardless of the distance of the object which produces it. The experimental work bears this out no more than the work already cited on acuity bears out a similar assumption with regard to the resolution of letters and figures.

The following summary is given of the data of one *O*. *Red:* small square recognized at 21 m., large square at 160 m., giving a ratio of 1:7.6; *blue:* small square recognized at 10 m., large square at 50 m., ratio 1:5; *green:* small square recognized at 9 m., large at 30 m., ratio 1:3.3; *yellow:* small square recognized at 28 m., large at 190 m., ratio 1:7; *orange:* small square recognized at 21 m., large at 150 m., giving a ratio of 1:7. Similar results were obtained from the other *O*s.

For the present these facts can only be presented without comment. As far as the writer knows nothing is available to explain the anomaly existing between the perception of the large and the perception of the small squares of color.

APPARATUS

A Silencer for Laboratory Keys

By S. M. Newhall, Yale University

The silencer is merely a quiet contact which can be substituted for the regular lower contact of an ordinary electrical key. Fig. 1 shows a key with the original contact removed and the special unit bolted in the old hole at (A). It is best adapted to keys of this unpivoted spring lever type not requiring a stop to limit the lever movement on release. Keys like the one pictured do have a stop but it may be screwed up out of the way and not used. Keys of the pivoted lever type necessitate silencing of the impact at the stop when the key is released. A thin rubber cushion such as a rubber band is effective and it may be placed either on the stop itself or on the lever. With either type there must be usually some readjustment of the separation of the contacts after installing the silencer, but that is very simply achieved by turning the adjustment screw carrying the upper contact.

Design and materials take account of two causes of noise, namely, impact on the make and arcing on the break. The electrical contact is of platinum attached to the center of a spring brass strip. The latter is bent to a rectangular form and the two ends fastened to the flattened head of a small bolt as shown at (B). The platinum is thin to limit the jar of impact at the moment of contact. The brass is also thin and it is cut with the grain so that it will yield for the same reason. It has large surface relative to thickness to give lower vibration rate since that tends to raise the intensive threshold. The frame construction helps to avoid slow, ragged break from excessive yielding, and a fairly light pressure in operating the key is desirable for the same reason. Adjustment of the contacts to small clearance when open favors light pressure and slight impact. While the yielding of the lower contact is disadvantageous in that it may increase the latency or variability of the break, there is, fortunately, little or no such effect on the make. The function of the rubber cushion contained within the brass frame is to absorb the vibration of impact. Any slight rattle that may be noticed is probably due to a loose binding screw or lock-nut and of course can be corrected by tightening.

Constructional details which have proven effective are given below. The spring strip is 60 mm. x 5 mm. cut from 33 B & S gage stock. This gives a cross section of over 2000 cir. mils which is sufficient to eliminate difficulty from heating. The strip is bent transversely and at right-angles in four places to form the closed rectangle, 25 mm. x 5 mm., with the ends meeting in the middle of one of the longer sides. Obviously, two of the bends are at 12.5 mm. from the respective ends of the strip and the other two at 17.5 mm. A round-headed 10 x 32 brass machine screw, cut to suitable length, serves as the attachment bolt. I used this size because it fitted the hole from which the original contact was removed. The head of the screw is filed down to about 1 mm. thickness and to the flat surface thus formed the free ends of the rec-

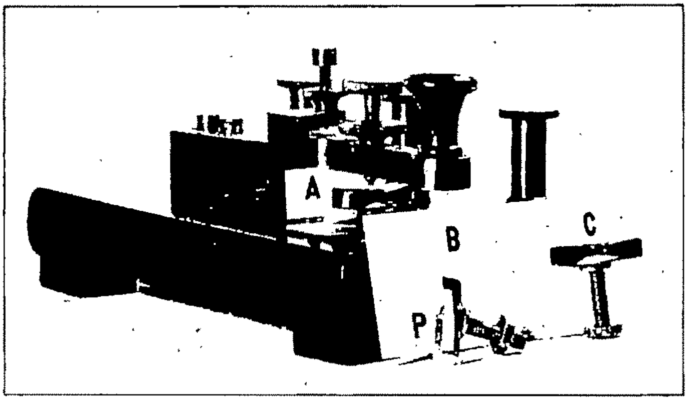

Fig. 1. Laboratory Key with Silencer

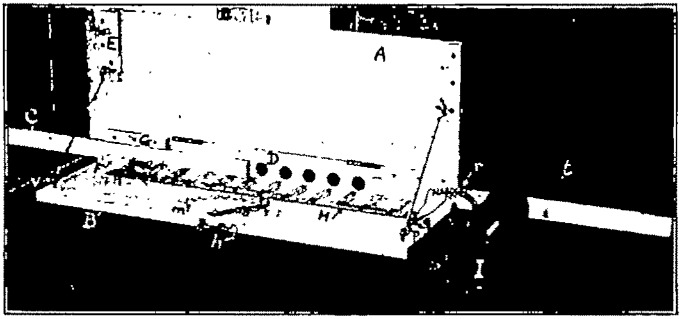

Fig. 2. The Multiple-Choice Apparatus
(Seen from the operator's side)

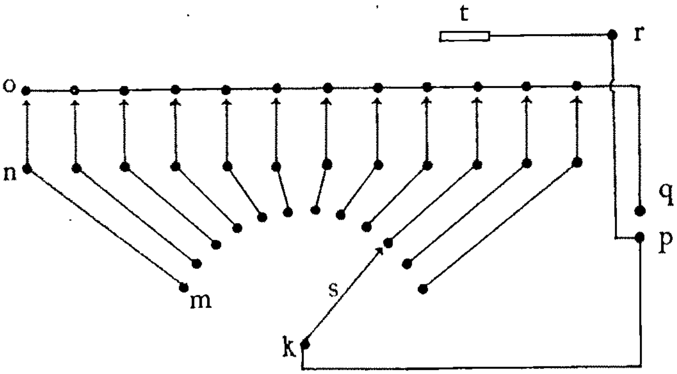

Fig. 3. Wiring Diagram of the Multiple-Choice Apparatus

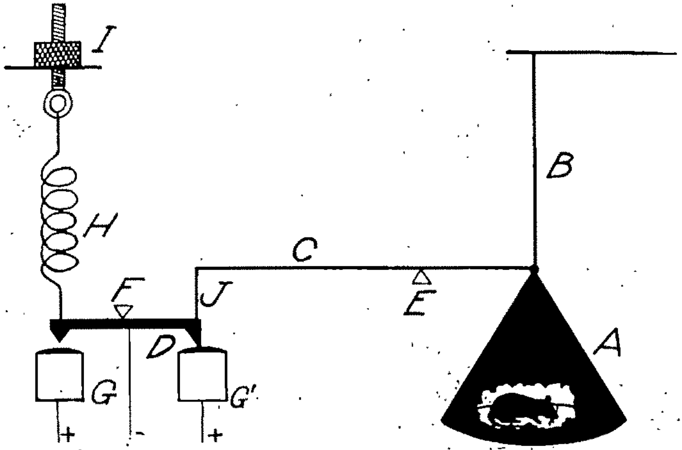

FIG. 4. DIAGRAM OF CONTACT AND SUSPENSION DEVICE

A, Suspension basket; B, rubber band; C, lever, 34 cm. long; D, lever, 5 cm. long; E and F, fulcrums; G, G′ mercury contact cups; H, small coil spring; I, screw for adjusting coil spring; J, strong thread about 2 cm. long joining the two levers. A slight downward push at A causes the electrical circuit to be closed at G, or broken at G′.

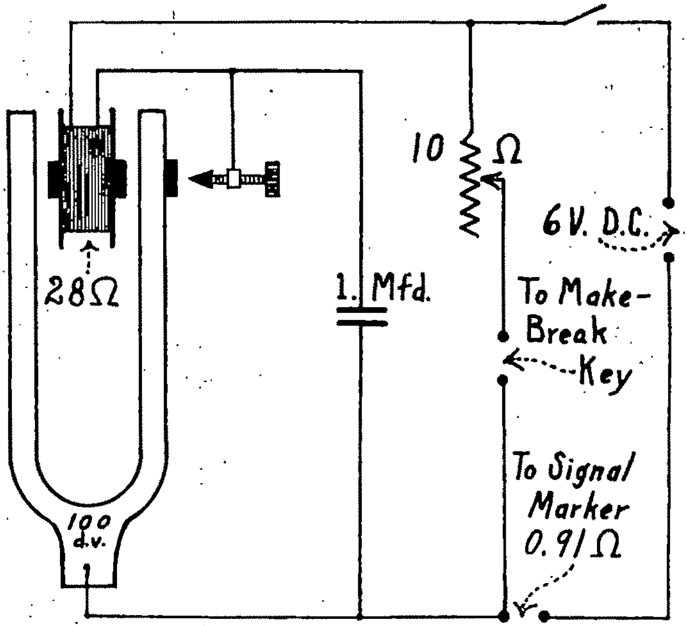

FIG. 5. WIRING DETAIL FOR THE TRIPLE-DUTY SIGNAL MARKER CONTROL

tangular frame are securely soldered. For electrical contact, a piece of sheet platinum about 5 mm. square is sweated to the long side of the frame in line with the axis of the bolt, as indicated at (P). The cushion is cut from a Goodyear size 30 red rubber eraser, 10 mm. wide and of such length and thickness as to fit snugly but not tightly into the frame. An arch-shaped portion about 13 mm. long is cut away from the under side of the cushion to increase its resiliency. This is shown at (A) or (C). The action of the frame is improved by bending the upper side to a slight concavity and the lower side to a slight convexity as shown at (A), (B), or (C).

The silencer is adapted to laboratory circuits of small inductance and current such as commonly used to operate a relay or electromagnetic recorder. When the silenced key was tested for audibility by making or breaking the circuit through the coils of a 20 ohm or 175 ohm relay, the results were satisfactory even with more than enough current to give positive operation of the instruments. Only a few observations were made but the observers knew what kind of sound to expect, when to expect it, the ear was normal to the source, and the room was fairly quiet. Under the conditions the limit of audibility seemed to be of the order of 50 cm. when the key was operated without special care and 10 cm. or less when the manipulation was rather slow. Though a slight spark could be seen in the dark room, under no circumstances was it aurally noticeable. Therefore, if some circuit should produce an objectionable arc at the key contacts, that could be eliminated by opening or closing the circuit through a relay so placed or mounted as to be inaudible to the observer. That method has the advantage of its certainty over the simpler one of shunting a condenser directly around the key. The quiet contact described has the advantages of simplicity in design, ease of installation, and freedom from mercury or other fluid.

A MULTIPLE-CHOICE APPARATUS

By K. E. ZENER, Duke University, and E. G. WEVER, Princeton University

The Multiple-Choice Method, developed by Yerkes from a type of problem which Hamilton devised for the testing of mental defectives,[1] has proved most valuable in the study of behavior of the problem-solving sort. Briefly stated, this method involves the presentation to the subject of a number of mechanisms, one or more of which are to be operated for the solution of a given problem; the problem consists in the discovery of the proper mechanism to operate in successive trials. Typical problems may require response to the right-hand member of the series of mechanisms exposed, or the left-hand member, or the middle member, etc.

The method has perhaps been most extensively used in the investigation of problem-solving in animals, though some application has been made to human adults and children, both normal and defective. The usefulness of the method in the investigation of normal human adult responses has been signally enhanced by the work of Brown and Whittell, who have developed a graded series of problems to be set before the subject in order of difficulty.[2] These investigators thus have elaborated a valuable instrument for research purposes. Incidentally they have also afforded us an experiment suitable for elementary laboratory classes in psychology, and an experiment that is as interesting as it is instructive.

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[1] R. M. Yerkes, A new method for studying ideational behavior, *J. Comp. Psychol.*, 1, 1921, 369-394.

In giving the experiment to a laboratory section of any size there of course arises the serious problem of equipment; this problem the writers have attempted to solve by designing a model of multiple-choice apparatus that can be made up in quantity at a reasonable cost.

Description of the apparatus. The apparatus is shown, from the *E*'s side, in Fig. 2. *S* sits behind the apparatus, holding in one hand the brass electrode (t). His view of *E*'s manipulations is obscured by the board (A). The holders (E) are provided for the insertion of a sheet of heavy cardboard for more complete screening than the board A affords, which may be found necessary if unruly *S*s such as children or abnormals are being tested; normally the cardboard will not be needed. Before *S*, in the board (D), are twelve 1-in. holes, any number of which may be exposed at one time. The two wooden sliders (C) are provided for varying the number of holes exposed. A finger extended through one of the holes will operate a key, which is simply a brass strip (n) mounted above a screw-head (o) so as to touch it when depressed. Pressing a key completes an electric circuit and gives *S* a shock in the operating finger, unless the response is the 'correct' one, in which case a short-circuit of the current will have been provided by a setting of the lever (s). This lever may be set so as to make contact with any one of twelve screw-heads (m), and thus short-circuit any one of the keys.

An understanding of the electrical connections will be facilitated by an examination of the wiring diagram in Fig. 3. Most of the wiring runs beneath the board (B), and is concealed from view. From (q), which connects with one side of the electrical supply, a wire runs to each of the screws (o) with which the keys make contact. From (p), which connects with the other side of the electrical supply, one wire runs to the electrode (t), and another to the screw (k) on which the lever (s) is pivoted. The screws (m) that are arranged in the arc of the lever's swing are each connected with a corresponding key. When the lever is turned to a given screw, it is plain that pressing the key to which it corresponds will give no shock, since the current will take the path of lower resistance. Pressing any other key, on the other hand, will lead to punishment in the form of shock.

This instrument is simple enough in operation so that the beginning student has no trouble with it and rarely makes a wrong setting. The screw-heads (m) are plainly numbered from 1 to 12 to correspond with the keys, and every *E* is supplied with a direction-sheet containing the problems and exact settings to be made. We have followed Brown and Whittell's list of settings.[3]

The apparatus has been made to fold up when not in use, and can be stored in a small space—an important consideration in most laboratories.

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[2] Warner Brown and F. Whittell, Yerkes' multiple choice method with human adults, *ibid.*, 3, 1923, 305-318; esp. 311-2.

[3] *Op. cit.*, 311-2.

AN APPARATUS FOR MEASURING THE REACTION-TIME OF WHITE RATS TO NOISE

By MARTIN F. FRITZ, Iowa State College

Every one working with rats has observed that they jump quickly whenever sudden noises occur. In order to measure the reaction-time of this movement the author devised the apparatus shown in Fig. 4.

The experimental animal is placed in basket (A)—an aluminum disk suspended by three wires and covered with black cloth—which is supported by a rubber band (B). The strength and length of the rubber band is varied according to the weight of the rat so as to bring the lever systems (C) and (D), to which the basket is also attached, into balance. Lever (C) is connected to lever (D) by a string (J). String (J) is kept taut by the action of spring (H), the tension of which is regulated by a set screw (I). Lever (F) acts as a double switch, making a circuit at mercury cup (G) and breaking a circuit at mercury cup (G′). The apparatus may consequently be used with any of the standard chronoscopes or chronographs with either a 'make' or a 'break' circuit.

The stimulus, which should be a sudden sharp noise of short duration, can be given by means of the Wundt sound-hammer, or by any apparatus that will produce a noise and at the same time make or break an electrical circuit. The author used a mouse trap adapted to make an electrical circuit when it snapped.

The reaction-apparatus can be adjusted by means of the rubber bands (B) and the set screw (I) for the registration of very slight movements. For example, in the apparatus that the author used, a downward pull on the basket (A) of 1.9 grm., which moved it through a distance of approximately 2 mm., was sufficient to make or break the electrical circuit.

The author believes that this apparatus, besides measuring the rat's reaction-time, will prove useful in testing the effects of drugs and diets, of training and fatigue, etc. He feels, from his experience with it, that it will measure more delicately and more accurately the influence of these factors upon the nervous system and organic condition of the animal than either the maze or the problem box.

A TRIPLE-DUTY SINGLE MAGNETIC MARKER

By SAMUEL RENSHAW, Ohio State University

Fig. 5 shows a wiring diagram for a single magnetic marker, fork-controlled, which does triple duty. It indicates the time of make and break and records in hundredths of a second the interval between these points. In our laboratory we have found the device useful for the chronographic measurement of reaction-times, for timing tachistoscopic exposures, etc. The marker indicates without appreciable lag the instant of make, the duration of the closing of the circuit in hundredths of a second, and the instant of the break on a single line on a kymograph. The magnet of the marker is arranged so that it is oscillating continuously at a very low amplitude. The make consists simply in stepping up the current to this marker by by-passing it around a variable resistance.

The 100 d.v. fork oscillates continuously, driven from a 6-volt D.C. source. The coil activating the fork has a resistance, in our instrument, of 28 ohms. The fork contact is of the bridge type, and is patterned after the one used by the Stoelting Co. in their control forks for the Dunlap chronoscope. A one m.f. fixed condenser is shunted about the breaker point on the fork to eliminate arcing. The small magnetic marker which we use is of the type made by Ebersbach of Ann Arbor, Michigan, and is wound to a resistance of 0.91 ohms. This marker is fitted with a small spring-steel armature of high period. The variable resistance of 10 ohms is adjusted by the experimenter so as to dampen appropriately the amplitude of the marker while the fork is in constant oscillation and before the circuit is made; for example, by the depression of the experimenter's key in a reaction-time experiment. Closing this circuit sends more current to the marker, thus increasing its amplitude; and breaking the circuit by the reactor's key restores the marker to its original status of oscillating at very low amplitude.

We have mounted these units on an oak board 12 by 12 inches. They have given us highly satisfactory results in the uses indicated above.

A somewhat similar device was described by Titchener.[1] The present device is a modification and improvement of the same principle originally used by W. G. Smith.[2] The points of difference may be observed by comparing the diagram of Smith's hook-up with that of the above figure.

Some New Auditory Apparatus

By Forrest L. Dimmick, Hobart College

During the past year the author has been using in the Hobart laboratory an instrument—the wiring diagram of which is shown in Fig. 6—for the production of relatively pure tones that covers a wider range than the full set of Stern variators, by the manipulation of at most two controls (a coarse and a fine adjustment) and that provides for a complete gradation of intensities from zero to maximum. The instrument consists essentially of two vacuum tube oscillating circuits one of which is fixed to oscillate at about 60 kilo-cycles while the other is variable between the approximate limits of 50 and 60 kilo-cycles. The beat-frequency between the two oscillating circuits is picked up and put through a detector and then an amplifier stage, thus emerging as a wave of voltage intensity whose rate depends upon the difference in rate of the two oscillating circuits. Any loud speaker whose leads are attached to the output terminals gives the corresponding tones. With a one-half turn of the dial one can go from the lowest audible tone to one near the upper limit. The manufacturers give the limits as approximately 20 to 9,000 cycles. The two instruments in the Hobart laboratory both exceed this range, especially at the lower end of the scale. Steady pulsations as slow as 4 to 5 per sec. can be heard with a telephone head set, though they are too weak to be audible on the loud speaker. Thus the lower limit seems to be set by the capacity of the speaker.

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[1] E. B. Titchener, *Experimental Psychology, Quantitative Experiments, Instructor's Manual*, 1905, 338 f.

[2] W. G. Smith, *Proc. Physiol. Soc.*, Nov. 17, 1894.

According to the manufacturers the form of the output wave is very nearly sinusoidal; "the total of harmonics being at a maximum but 4% of the wave in voltage." We have found this to be approximately true with both of our instruments below a tone of about 5000 vs. Above that some harmonics are audible. We have not yet attempted to determine the reason for this or to eliminate it. Variations of intensity are obtained by means of a potentiometer which is bridged across the output. A turn of about 300 degrees gives the full range of intensities available. One of our instruments is the commercial model put out by the General Radio Company. The other we were able to build in our own laboratory thanks to the courtesy of the manufacturers who furnished the necessary specifications. Both instruments are equally satisfactory. The commercial model is designed to use either dry cells or external batteries. Equipped with the former, it is completely self-contained except for a loud speaker and fully portable. It is, therefore, excellent for lecture demonstrations. If the instrument is to be used for research purposes, addi-

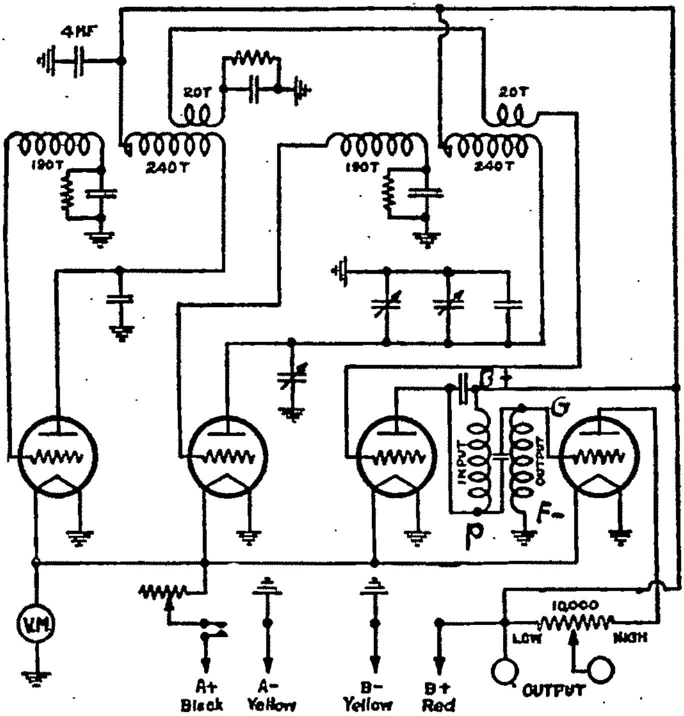

FIG. 6. BEAT-FREQUENCY OSCILLATOR
(Type 413, General Radio Co.)

tional equipment may be required to suit the particular experiment. For example, we have found it necessary to add a graduated dial to the intensity adjustment, and to provide a special switch in the leads to the speaker. For the latter any one of the better grades of instruments will serve. The author uses a 560 AW Westinghouse cone.

In order to turn the current on and off into the loud speaker, we have resorted to the following device. The output from the oscillator is carried first to a General Radio speaker filter number 387 A, and the output from this is bridged by a 10,000 ohm potentiometer. The speaker leads are connected to one side of the filter output and to the moving member of the potentiometer. By means of a small gear on the stem of the potentiometer which meshes with a larger geared quadrant on a lever arm the intensity of the output into the speaker is raised from zero to maximum in a fraction of a second. This eliminates the click in the speaker that occurs when an ordinary contact is made or broken.

ADDITIONAL IMPROVEMENTS OF THE SANFORD CHRONOSCOPE

By CHRISTIAN A. RUCKMICK, University of Iowa

Dr. Dimmick's further improvement of the Sanford chronoscope is decidedly a step in the right direction.[1] The substitution of the metal rods for the troublesome threads in the older forms might easily be combined with the magnetic release suggested by me in a previous note.[2]

I should like, however, to suggest three additional modifications which appear to make the apparatus still more dependable and convenient. (1) The main hexagonal adjusting-nut should be replaced with a knurled thumb-screw bearing on its upper surface punch-marks in divisions of tenths of the circumference so as to make possible practically a micrometer adjustment. (2) The pendulum-rods should be made of hard rolled sheet-brass further to reduce the air resistance as is frequently done in clock pendulums. (3) A slotted and hinged arm with some slight spring pressure at the hinge should be attached on that side of the upright support of the chronoscope proximal to the pendulums. This could be released to the horizontal position just above the bobs, thus keeping them in the vertical position when the chronoscope is moved from place to place in the laboratory.

The Sanford chronoscope is still a universally used instrument in our laboratories and shows such promising service in connection with the development of laboratory sections in our elementary classes that improvements of the type outlined in Dr. Dimmick's note are very much worth while. Of course at the same time the modifications must be made as simply as possible in order to keep the instrument within the purchasing power of our smaller academic institutions as well as within the multiplied requirements of our larger universities.

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[1] F. L. Dimmick, A further modification of the Sanford chronoscope, this JOURNAL, 41, 1929, 475-476.

[2] C. A. Ruckmick, Some suggestions in laboratory apparatus, *ibid.*, 38, 1927, 647-648.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Colour and Colour Theories. By CHRISTINE LADD-FRANKLIN. Introduction by C. K. Ogden. New York, Harcourt, Brace & Co., 1929. Pp. xv, 287. Nine colored plates.

This volume is made up of the collected papers of Dr. Ladd-Franklin, reprinted from various sources, with minor alterations and additions. Quite frankly (as announced by the editor in the preface) the book pivots upon the 'molecular dissociation' theory of color vison, launched in 1892 at the International Congress of Experimental Psychology in London, as a suggested *rapprochement* between the rival factions of Hering and Helmholtz. This statement, together with the list of 'contents,' will doubtless provoke a momentary pang of disappointment in the reader, hopeful of an ordered résumé of the rapidly expanding mass of visual research, a substitute for the inadequate revision of Parsons' *Introduction.* So utilitarian and impersonal a product, however, lies outside the apparent aim and province of the book before us.

While the Ladd-Franklin theory (owing probably to the uniqueness of its evolutionary features) finds its way annually into an increasing number of texts, few expositions do justice to the subtlety of its original framework and basic chemical postulates. With the initial sketches before us (the requisite emendations indicated usually—not always—by bracketed footnotes and inserts), the groundplan and superstructure of the theory as a whole is clearly manifest, and *ipso facto* the baselessness of many of the strictures passed upon it by Parsons, Troland, and others. One wonders, indeed, as one reads, whether its critics have ever encountered it except at second hand. The reviewer, for one, admits to receiving for the first time, from the perusal of this volume, an adequate conception of the theory, with all its merits and defects.

The book itself, to be sure, provides no single integrated exposition, either of the facts of color vision, of the 'genetic' theory in revised version, or of other theories. Instead, we are confronted by a somewhat disjointed series of 20 or more papers, covering a period of about thirty-five years, from 1892 to 1926; reprinted from *Mind, Science, Nature, Psychological Review,* the *Proceedings of the International Congress of Psychology,* and the like, and including an article on Vision from the *Dictionary of Philosophy and Psychology,* and the Appendix on Color-Sensation from the recent American edition of the *Physiological Optics* of Helmholtz.

The assemblage and collocation of the *membra disjecta* of the theory therefore devolves upon the reader. In spite of the vigor and clarity of diction of the author the task is not an easy one. Nor is it facilitated by certain formal defects in the edition; *e.g.* the lack of typographical uniformity in the serial numbering of papers and plates in the text, and in the Appendix to which one must turn for the date and place of publication, as well as for the description of the plates. The misprinting of technical terms and proper names is another minor irritant: one picks at random Herzing for Hering, Graeffe for Greeff, 'deulenanope,' 'telrachromatic,' etc. The substitution of phanerogamic for cryptogamic (p. 143) is doubtless a *lapsus calami* on the part of the author; so also the inversion of Donders' postulates in five out of six references to his theory in the text.

Many of the papers reprinted are controversial in tone, protesting the unacknowledge appropriation of portions of the theory, or of one of the author's two original experimental contributions (the demonstration of the normal night-blindness of the fovea, and the disproof of Newton's law of the equivalence of equated lights in all illuminations) by Schenck, König, von Kries, Ebbinghaus and others. The charge of lack of originality, of an unacknowledged debt to Donders is amply clarified—and quashed—by the data here available. The indebtedness to Donders (of whom after all Ladd-Franklin borrowed no more than Helmholtz did of Young, or Hering of Mach) is definitely recognized in at least six papers, including the first report before the Congress. In five of these, to be sure, the basic postulates of the Dutch opthalmologist are curiously reversed, by no means to his credit. In all, however, the use of an evolving color-molecule as a device for harmonizing trichromatic and tetrachromatic findings is frankly traced to his inspiration.

A number of the contributions take the form of a rebuttal of objections, adducing supplemental evidence from recent discoveries in chemistry, histology, and related disciplines. The astuteness and soundness of the original postulates, which have weathered thirty-seven years with minor alterations only, is increasingly apparent. The so-called 'mythical' visual molecule, with its three states or stages of development, is shown, upon the testimony of experts, to be chemically unassailable; a parallel substance, a rosaniline carboxylate has even been discovered. The positing of a single photo-chemical substance, present in different stages of development in rods and cones, and of a single tri-process receptor in the central retina, is substantiated by the later findings of Cajal, of Greeff, and of Hecht, and by certain blue-arc experiments of Ellis, reported in these pages.

When the last word is said, however, the theory leaves the impression on the reader of a logical rather than a psychological *tour de force*. Inspired by successive semesters with G. E. Müller and König, the primary purpose of the author, *i.e.* the effecting of a compromise between the reigning 'components' and 'opponents' schools, has obviously failed of fulfillment. After thirty-seven years of spirited polemic on the part of Dr. Ladd-Franklin, the followers of Hering are still insensible of the cogency of the König color triangle (except as a matter of economy in certain laboratory techniques). The disciples of Helmholtz, also (with minor exceptions), are as 'pre-psychological' as their former leader; as 'psychically blind' to yellow in their accounts of color-blindness; as naïvely confident in the power of mind to achieve the miracle of white from a blend of red-green-blue. In this latter obduracy, indeed, the physicist is confirmed by echoes from the outskirts of psychology itself: the asseverations of McDougall as to the synthetic abilities of the psyche; the easily distorted axiom of *Gestalt*, which asserts the inequivalence of the whole and its parts.

After long and careful scrutiny, the laboratory psychologist is compelled to adjudge the postulates of Ladd-Franklin as little satisfactory as the discarded ones of Helmholtz. The explanations of color-blindness discoverable in these pages are sketchy, involved, and little helpful. The psychological premises themselves on which the older theories are attacked and the superiority of the genetic hypothesis advanced are less compelling than in the nineties. Reliance upon the 'immediate deliverances of consciousness' in the selection of color 'primaries' fell away in the early nineteen-hundreds. Recognition of the simplicity of all color-tones (as such) long since obviated the objection to Hering's 'inexplicable choice' of 'bluish-red' (crimson) and 'bluish-green' (verdigris) as his *Urfarben* (as 'simple' undoubtedly to him—and to most of us—as the lithium or thallium bands of the spectrum to the ordinary eye sophisticated by commercial dyes). Nor is the doctrine of 'psychophysical parallelism' (the obligation to posit similar physical causes for similar psychical effects, *e.g.* for twilight and daylight grays) as sacrosanct as in the decades nearer Fechner. Indeed, the theorist here has been unable to keep faith with her own cardinal principles; abandoning the spectral red for one 'less yellowish,' shifting her green toward verdigris with the passage of the years (oblivious of the fact that the very corner-stone of her system—the generation of yellow by the mixture of orthodox 'unitary' spectral red and green—is thereby subverted). Her insistence upon the 'non-light' cortical or 'background' character of black, in spite of its obviously visual texture and quality, constitutes a virtual abrogation of her second principle, a rigidly interpreted parallelism.

The enduring value of Ladd-Franklin's contribution to visual science, one suspects, lies less in her solution of the Helmholtzian enigma of yellow or of white (the mystery of the 'vanishing color-pairs') through the reversion of the color-molecules, than in her attempt to place both scotopic and photopic vision upon a single unified basis, explicable in evolutionary terms. Her valiant campaign to convert physicist and opthalmologist to a less promiscuous use of stimulus and experiential terms in description also merits gratitude. In the Glossary attached to this volume numerous modifications of current usage, essential if the findings of related sciences are to be mutually available, are suggested: *e.g.* the substitution of achromatic for monochromatic in the designation of total color-blindesss; of *xantho-* or *erythrogenic* radiations for 'yellow' or 'red' wave-lengths when stimuli, not sense-*qualia*, are in question.

A new and striking series of colored plates, distributed through the text (in addition to numerous cuts in black and white), augment one of the chief assets of the theory (according to certain critics), its pedagogic ease of presentation, in schematic form. The plates, to be sure, are of unequal merit, both as regards color-printing, and illustrative design. Plate II, a modified reproduction in color of the König-Helmholtz analysis of the spectrum, reveals apparently too much; suggesting to the reviewer's mind a number of hitherto unnoted inconsistencies in the theory. Plate VIII, showing the color triangle of the tetrachromatic and of the two types of red-green blindness—the 'deuteroxanthic' and the 'protoxanthic'—should prove an eye-opener to certain unreconstructed Helmholtzians. Actually, however, it renders more patent a mystery which would seem to demand a complete recasting of the theory, or the 'color-molecule.' As it stands the theory explains deuteroxanthia only. As for the articles by other writers, inserted at the end of the book, these appear to add little to the argument, and are below the general scholarly level of the original contributions.

Apart from its permanent scientific worth, the volume is of interest in the side-lights that it throws upon past feuds and schisms still effective in directing the currents of visual research and theory. Particularly illuminating are the sections referring to the era when the recently discovered properties of 'visual purple' gave rise to a ferment of theories afterwards discarded. Whether describing the arrival of an excised human retina at König's laboratory, inventing new illustrative analogies to startle the dull-minded, or badgering her critics, Ladd-Franklin is never dull. The newcomer, especially, in the psychological field will find *Colour and Colour Theories* a welcome addition to his shelves.

Cornell University ELSIE MURRAY

Common Principles in Psychology and Physiology. By JOHN T. MACCURDY. New York, Macmillan Co., 1928. Pp. xvii, 284.

The author states that his object is to suggest a vocabulary in which the principles common to reflex action, instincts and habits, and conscious and unconscious mental activities may be expressed. He actually succeeds in formulating a comprehensive, non-materialistic system of psychobiology. This system was anticipated in his previous study of emotion, but in the present book it embraces in detail the basic principles of both psychology and biology.

MacCurdy postulates the existence of specific immaterial agencies in biological phenomena. These agencies are 'patterns,' and they are due originally to the tendency of all stimuli which effect the organism simultaneously or successively to become united. The result of the initial stimulation is a lowered threshold for one or more of the stimuli. With sufficient repetition the threshold falls to zero, and responses are then made to image 'functions.' The latter are capable of integration into new patterns. All psychological phenomena are to be explained in terms of the integration and activation of patterns.

MacCurdy's 'image' resembles, but it should not be confused with Semon's 'mneme.' Semon, Loeb, and others who believe in some form of protoplasmic 'memory' dispense ultimately with the immaterial factor; but MacCurdy cannot accept this final mechanistic explanation. He regards the immaterial factor as a scientific abstraction, comparable with the abstractions of mathematics and physics. To postulate a factor which functions consistently as an immaterial agent, and then to explain it in physico-chemical terms is illogical. On the other hand, MacCurdy's image is not spiritistic. It is usually subliminal, but at times it is discoverable introspectively. It is not, however, a ghost.

The author's conception of image functions is most clearly explained in his references to behavioristic psychology. To use his illustration, a conditioned response is shown by the man who cowers in the presence of his enemy, for we cannot assume that this individual has, necessarily, a conscious image of the previous beating. But MacCurdy argues that in the conditioned response the organism acts as if a stimulus experienced in the past were again in operation. For example, he finds that Hunter's 'sensory thought' is in reality his image function, on the grounds that if proprioceptive stimuli are employed after the muscles have been engaged in another response these stimuli must be of an imaginal nature. In general the chain reflex, sponsored by certain behaviorists, is inadequate because it places the organism entirely at the mercy of its environment. But a considerable sympathy for objectivity in psychological thinking is reflected in MacCurdy's theory, in that image functions can be tested objectively. An imaginal process which leads to an immediate response is maladaptive—as when a dog salivates on hearing a whistle, or when a human has hallucinations. But when an image functions indirectly—when, for instance, the dog runs toward the source of food—the action is adaptive. Consciousness is the capacity of the organism to be aware of its own thoughts; objectively, this means the capacity to discriminate between stimuli arising in the present environment and stimuli occurring as reproductions or elaborations of past experience.

Possibly the outstanding feature of MacCurdy's psychology is its affiliation with the *Gestalt* hypothesis. Reaction patterns are images which serve as stimuli for separate elements in overt behavior in the absence of present, material stimuli which ordinarily would evoke the response. Images form, by permitting substitutes, new patterns independently of experience. Attention, perception, learning, and meaning are explained in terms of image function and pattern activation. The motivating factors are by no means ignored; instinct means not only reacting to a stimulus but seeking a stimulus. The latter phenomenon is due to a lowered threshold for a particular pattern. It will be noticed that MacCurdy's patterns, as distinguished from the *Gestalten* of the German schools, are acquired by the integrative process.

The second part of the book is devoted to an interpretation of biological phenomena in terms of the image function theory. Of particular interest to psychologists is MacCurdy's interpretation of central nervous system functions. The basic functions are patterns which can be accounted for only by the assumption of image functions. In brief, they are products of relationships among points of excitation or stimulation; they are, in fact, anatomical 'designs' which are three- and often four-dimensional, for they unfold temporally. At any given time, much of the central nervous system is inactive. Inhibition and repression, as well as other phenomena, find an explanation in the concepts of partial and preliminary activity of dominating patterns.

Because of its scope, this book cannot be characterized adequately in a brief review. Perhaps the chief psychological contribution of its author is his attempt to express the standpoints of functional, behavioristic, purposive, and *Gestalt* psychology, respectively, in terms of his own pattern psychology. A book which purports to accomplish such a task need not be entirely convincing in order to be profoundly stimulating.

University of Texas F. A. C. PERRIN

Gestalt und Sinn. Edited by FELIX KRUEGER and FRIEDRICH SANDER. *Neue Psychologische Studien,* Vierter Band, Erstes Heft. München, C. H. Beck'sche Verlagsbuchhandlung, 1928. Pp. 159.

This number of the *Neue Psychologische Studien,* a periodical edited by Professor Krueger, contains the first two of a series of studies dealing with the general problem of the relationship between configuration and 'sensibility.' The titles of these two studies, with their respective authors, are: (1) Elisabeth Lippert, *Unterschiedsempfindlichkeit bei motorischen Gestaltbildungen des Armes* (pp. 1-83); (2) Carl Schneider, *Untersuchung über die Unterschiedsempfindlichkeit verschieden gegliederter optischen Gestalten* (pp. 86-159).

(1) Miss Lippert reports an interesting series of experiments, showing, in general, that discriminability for a change in the extent of two arm movements made in succession by a subject varies with the degree of unitariness or *gestalt* inherent in the type of movement. She studied three types of movement: (1) a single 'straight-line' movement, (2) continuous movement in a straight line (*i.e.* back and forth in a straight line), and (3) 'round' movement of four sorts (in a circle, in a vertical ellipse, in a standing ellipse and in the form of an unsymmetrical figure). The three varieties of movement are ranked according to unitariness by the author in the order just given, the single straight movement having the least and the curved movement having the greatest 'unity.' The introspections of the subjects confirm this ranking. The quantitative results show, correspondingly, that sensitivity to a change in amount of movement is greatest in the case of the round form (and greatest of all for the circle), intermediate for continuous, straight-line movement, and least for a single straight-line movement. At least, the figures show this after the rather peculiar manner in which the author reduces them to a comparable basis (see pp. 48-60).

(2) Schneider's problem is similar in many respects to that of Miss Lippert. He exposed to his subjects pairs of rectangles in succession for 120 sigma each, and asked them to report whether or not the second rectangle of a pair differed from the first with respect to *gestalt.* He used two general types of rectangles: (1) those 'ohne Binnengliederung', *i.e.* without 'internal organization' or interconnecting parts, and (2) rectangles with such internal organization. The first part of the study is devoted to experiments with 'plain' rectangles. All of the figures were 60 mm. in length, but they varied in height by steps of 5 mm. from 5 mm. to 100 mm. His problem was to find for each subject the limen for sensitivity to a difference in the form of the second of the pair of rectangles as compared with the first. His results show, generally, that discriminability is poorest for the square (60 × 60 mm.) and best for those rectangles approximating the dimensions usually given for the 'golden mean' in aesthetics, namely those with approximately the following dimensions, 38 × 60, and 60 × 98 mm (except for the figures below 20 mm. in height, in the case of which the constant change employed would be greater proportionately than for the higher rectangles; changes in height only appear to have been introduced in the experiment). These 'perfect' rectangles appear to have been the most pleasing to the subjects and changes in their size were most quickly noticed.

In the second part of the experiment rectangles with various kinds of interconnecting lines, *e.g.* diagonals, were used, and the discriminability compared with that for the plain figures. The figures with the 'internal organization' gave a much higher degree of discriminability than was found for the plain rectangles.

The experiments in these two studies have been carefully planned and executed. Although only a few subjects were used (eight, at most, in each study), the main general conclusions, based upon averages, are rarely reversed in the results of any subject. This is a pretty fair test of their accuracy. In general, this type of work, although somewhat limited in its scope, appears more promising than much of the experimentation by *Gestalt* psychologists of the school of Wertheimer.

Vanderbilt University LYLE H. LANIER

Phonophotography in Folk Music: American Negro Songs in New Notation. By MILTON METFESSEL. Introduction by Carl E. Seashore. Chapel Hill, Univ. North Carolina Press, 1928. Pp. x, 181.

In the introduction, Part I, Seashore gives a short historical orientation, quoting largely from his earlier papers presented before the National Academy of Sciences, and explains that the collection here presented is in no sense to be taken as a systematic collection of any type of music of any group of people. It is, on the other hand, only a few random samples intended merely as illustrative of certain possible methods of technique to employ in the recording and study of kinds of ethnic culture which have hitherto evaded scientific preservation. Moreover, the records in these songs are limited largely to only two (tonal and temporal) of the four fundamental aspects of the song, thus omitting the possibilities of amplitude and *timbre* variations. He pleads for the now-possible preservation of the primitive folk traits (now being obliterated) of our Indians, Negroes, Hawaiians, and Filipinos.

The author, a student of Seashore's, describes in Part II his methods of study—the apparatus, methods of recording and analysis, the new "pattern notation" (necessitated by the visual presentation of the detailed analysis), interval and tempo graphs—and defines certain terms to be employed. These descriptions and definitions must be gone over carefully by the reader, with applications to the illustrative selections of artistic singing and of speech, if he is to get the most out of the types of negro songs exhibited in the following part—the workaday religious songs, the more formal spirituals, work songs, 'blues,' and general songs. By means of different colors of ink the individual voices in a quartet, recorded one at a time in different renditions and analyzed in separate voice-curves, are brought together and shown in one staff for the study of harmony effects. Each time, the individual whose voice-curve is to be recorded steps up in his turn to the mouth-piece, while the other three voices, not strong enough to affect the camera, serve nevertheless to give the individual singer in question his bearings. A simpler and more accurate method, it is explained, would be to use wide photographic paper with a separate sound-wave taken for each voice, all simultaneously. The method used necessitated

Elementary Psychology. By ARTHUR I. GATES. Revised edition. New York, Macmillan Co., 1928. Pp. xvii, 612.

Although the revised edition of this book differs considerably from the first edition, much of the subject matter is the same, and the general point of view is unchanged. Even more than its predecessor, the revised edition is characterized by its richness of psychological content. In addition to its more extended accounts of various psychological activities, it contains chapters on personality, individuality, and efficiency. The student who masters it is indeed equipped with a background of general, as well as elementary, psychology.

The Reaction Hypothesis is the basic principle in the author's psychological scheme. It is introduced in the second chapter and is never lost sight of in subsequent discussions of psychological phenomena. For Gates this hypothesis is a pragmatic, and to some extent a methodological, principle: conscious states as well as glandular and muscular activities are reactions. He is thus enabled to adopt structuralistic, functionalistic, and behavioristic standpoints freely as occasions demand, without raising controversial issues.

Such catholicity, however, is attained necessarily at the expense of systematic explanation. Gates is interested primarily in the historical, descriptive, and cultural aspects of psychology. In his chapter on Thinking the author discusses, among many other topics, imagery, 'pure' ideas, motives in reasoning, methods of reasoning, and creative imagination; but he insufficiently explains the mechanism of thinking, and he ignores the one phase of the process which has contributed much to our knowledge of this mechanism; namely, the genetic phase. The author's discussion of Learning goes to the other extreme in that it embraces a number of explanatory principles: both 'Satisfaction' and 'Annoyance,' and joint (conditioned) responses are postulated. It should be said that they are advanced as descriptions of the conditions under which learning takes place. Likewise the account of Intelligence, liberally supplied with factual material, omits any reference to the Spearman-Thorndike issue. In all discussions of higher functions, fundamental explanation is subordinated to description.

The difficulty involved in writing a text which seeks to reconcile traditional material with recent scientific research is reflected in the author's discussion of the nervous system. The theory of synaptic resistance is offered, with little critical reservation, to explain how and why impulses become directed into appropriate pathways. The revised edition is, however, more conservative than the first regarding cerebral functions. The conclusions of Franz and Lashley, omitted from the text of the original edition, are introduced to offset the attempt to correlate sensation, perception, and thinking with circumscribed cortical areas. The reader infers that the cerebrum is not only a connecting device, but the 'organ of thought' as well.

At times the author's interest in classification can be justified more readily on pedagogical than on scientific grounds. For example, while he points out that an 'urge' is not a single and an indivisible entity, his list includes the urges to collect and hoard, to fight for its own sake, and to hunt and destroy. The reviewer believes that writers of general texts could profitably devote less space to classifications of this sort and more space to discussions of the factors which underlie specific behavior trends.

Perhaps the chief criticism to be made of this really admirable text concerns its author's persistent tendency to distinguish too sharply between 'native' and 'acquired.' It is true that in most instances Gates is unable to classify a given act under either heading; but he seems desirous of so doing. He assumes that the two are logically independent factors. Such an antithetical distinction is questionable in the light of recent critical opinion on the subject. Certainly the psychologist should keep pace with the biologist in the attempt to formulate satisfactorially the issues involved.

University of Texas F. A. C. PERRIN

Educational Psychology. By A. M. JORDAN. New York, Henry Holt & Co., 1928. Pp. xiv, 460.

This book places great stress on data derived from studies of children in the practical schoolroom situation. The author is inclined to underestimate the value to educational psychology of numerous well-controlled investigations in animal, physiological, and experimental psychology. There is sometimes danger in basing conclusions and principles too much on results from situations that are so practical and so poorly controlled that they are purely 'local' and ephemeral. There is, however, no good reason for objection to the use of data derived from practical educational situations, so long as these data are obtained under properly controlled conditions and are checked up with results of experiments dealing with similar processes from different angles and on different individual subjects. Indeed such data are very desirable. The author puts a wholesome emphasis on quantitative and statistical handling of data from the beginning of the book, and, to aid the student in this line, he introduces a special chapter on elementary statistics. This might well have been given as an appendix for convenient reference, and the main facts involved could have been informally brought out in earlier chapters and exercises of the book. Like most educational psychology treatments, this book neglects giving considerable attention to the setting up and testing of hypotheses and to experimental procedures and the control of irrelevant factors. Nothing, for instance, is said of spurious correlations and of the low prediction values of rather high correlations. If we get more carefully obtained statistical data, we shall probably be in a surer way of advancement in psychology.

The general outline of the book is not far different from that of Thorndike's three volumes on the same subject, and the influence of this authority is obvious throughout. Part I is devoted to inherited nature and ability; learning, with application to elementary school subjects as well as to effective methods of study; transfer of training and transfers in school subjects. Part II covers statistical methods and problems relating to individual differences—the extent of differences; sexual, racial, and maturity factors; and influence of family and of environment. The next two parts, which complete the volume, are devoted to intelligence tests and educational tests and to problems of maladjustment and mental hygiene, respectively.

The book is written with a view to use as an elementary text for teaching purposes, and the different chapters contain simple questions and exercises (too general, in the main, probably), as well as references to other elementary works. The author accepts too uncritically the sufficiency of the so-called "laws of learning," originating with Thorndike, and the "normality" of distribution of scores. The reviewer believes that much elementary educational literature has taken up too naïvely certain earlier conceptions developed in the beginning of scientific psychology, largely because of their simplicity for pedagogical purposes, and that it is becoming merely indoctrinary to an amazing extent. There is too much unwillingness to accept probabilities as such and to acknowledge frankly the large elements of ignorance which we still have of many detailed aspects of behavior. Therein lies the danger, for how can attitudes favorable to research and inquiry grow up in such soil? Is education not becoming too much a process of "imparting information" rather than one of stimulating inquiry and critical evaluation of data by objective, scientific methods? The author has chosen to confine the topics of the book mainly to psychological rather than to educational topics, and he has produced a very clear and readable book for elementary work.

J. P.

La conscience morbide. By CHARLES BLONDEL. Second edition. Paris, Felix Alcan, 1928. Pp. 402.

The book is divided into three parts. In the first the author presents observations on seven clinical cases; in the second he generalizes about and systematically interprets his observations and in the last part he presents his theory of the morbid consciousness.

In all the cases that he discusses he finds a striking uniformity of behavior. This is especially clear in those reactions which show what he calls "the motor paradox." Inertia, the refusal of food, etc. are accompanied by scenes of despair and violent excitement. Correlated with this disorder is "the affective paradox."

Patients show great individual difference in their language behavior. The ability to express what they feel varies from a mere mouthing of meaningless phrases to a clear, lucid exposition. However, one is not to accept the latter too literally. By a process of construction and elimination Blondel has correlated the morbid images and the painful cenesthesia of the patients. His conclusion is that the bodily conditions are not the cause but the effect of the images.

As far as the intelligence of the patients is concerned Blondel asserts that there are great variations—from very low to rather high. Thus, absurdity of ideas is not an indication of feeble-mindedness. He postulates a mental state which is out of equilibrium.

In the ability to conceptualize there are also great individual differences, ranging from one patient to whom the past and present mean nothing—living in the presence of some future horror—to another who has no concept of time. Memory capacity varies from that of subjects who have a very well-defined consciousness of the past to that of those for whom the past is only a vague fantasy. Dominating all these other characteristics is the universal one, morbid anxiety. Blondel asks: "What is this anxiety?" He rejects physiological hypotheses, from both a practical and a systematic standpoint. There are no lesions which show any correlation.

He presents his theory from a socio-genetic point of view. Long analysis has convinced him that the morbid consciousness is different from the normal. The foundation of his theory is the sociological concept of collective ideas (*représentations collectives*). These are ideas which Lévy-Bruhl describes as being common to a given social group, transmitted from generation to generation, residing within the individual and giving rise to sentiments of respect, fear, adoration, etc. Because of this, thinking is socialized behavior. The perceptions of primitive people were enveloped in mystic elements which could not be differentiated, but as man developed, as his language developed, and as the sign language of his emotions developed thinking became exceedingly socialized. Back of all this complex behavior, however, there is the absolutely pure psychological state (*le psychologique pur*), cenesthesia, as opposed to those states of consciousness which have evolved out of it—conceptualizations as shown in words and bodily expressions. In the normal individual this cenesthesia is of a subconscious character, but in the abnormal it is a state of consciousness.

When the morbid consciousness is forced to make its troubles objective in words, this added trouble tends to become one of the elements which strengthens the other elements of the content. This is the condition of the "affective and motor paradoxes." The attempt at analysis of the mass only causes the mass to become more unanalyzable. Blondel, with this as a basis, expands the theory to fit particular cases. In an appendix consisting of two lectures he further presents his theory.

Psychologists eagerly receive any book which advances the scientific status of psycho-pathology. *La Conscience Morbide* is, unfortunately, not such a book. In its theory, which smacks strongly of purposive sociology, scientifically minded psychologists of the abnormal will have little interest. The book, however, has the virtue of being clear, and it may be valuable in its practical applications to work in the clinic.

Clark University — Clarence H. Graham

The Polish Peasant in Europe and America. By William I. Thomas and Florian Znaniecki. New York, Alfred A. Knopf, 1927. Two volumes. Pp. ix, 2250.

The new two-volume edition on the Polish peasant supplants the first five-volume edition published in the years 1918-20. It makes this classic of sociological literature more accessible to the increasing number of social psychologists, sociologists and social workers who find the work indispensable as a model of research and a source-book on methods of approach to the study and interpretation of social phenomena.

Never before and not since has a sociological study been made that in any way can match the scope and value of this treatise. It sets forth a novel and fruitful way not only for the analysis of the immigration problem but of group-life in general. An exhaustive treatment of the social life and the processes of disorganization and reorganization of the peasant community in Poland serves as a background for the detailed interpretation of the problems of adjustment which the peasant, as an immigrant, faces. A novel method is used by deriving material from first-hand sources; correspondence between members of family groups and autobiographical write-ups. The authors have also offered a new methodological basis for analysis by introducing the attitude-value concept, the four wishes, a new procedure for the development of laws and generalisations and valuable suggestions for the applied sociologist and social worker. From the methodological point of view alone Thomas' and Znaniecki's book is a pioneer work that is destined to play an important part in the development of scientific adequacy in the social sciences. The richness of fertile and poignant suggestions and comments which are to be found in profusion throughout the whole treatise, have not yet, however, been exploited and applied to a great extent, nor is their significance fully realized by the sociologist. It is to be hoped that the new edition will draw a renewed attention to this work.

It is to be regretted that the authors abstained from a revision of the treatise. The original was written over a period of years, during which points of view have somewhat shifted. This made for discrepancies in the text which the new edition should have corrected. Furthermore, the actual text does not show clearly the application of the principles of the "methodological note" which precedes it. The "note" was apparently conceived after the text was completed. But the value of the book could be considerably enhanced by relating with greater precision the conclusions of the text to the methodological propositions.

By way of comment it should be said that the relative contributions of the two authors has for some time been a matter of controversy. In view of the translations of Polish materials that constitute the bulk of the two volumes, and the thoroughgoing knowledge of the Polish background and the 'psychology' of the Polish peasant which the treatise shows, it is evident to the reviewer that Znaniecki has the greater share in the achievement. And so far as the theoretical aspects of the treatise are concerned, Znaniecki's recent publications indicate clearly that his influence in their formulation also was dominating.

University of Illinois THEODORE ABEL

Modern Psychology: Normal and Abnormal. By DANIEL BELL LEARY. Philadelphia and London, J. B. Lippincott Co., 1928. Pp. xiii, 441.

This volume is a text presenting a system of psychology from a strictly behavioristic point of view. The point of view is synthetic throughout and the author continuously emphasizes it as against that of analysis. It would seem to the reviewer that the sub-title of the book is more appropriate than the actual title. The book is more a discussion of personality and the factors influencing personality than a systematic discussion of modern psychology. For example, the whole problem of receptors, and with it the entire problem of sensory experience and perception, is dismissed in a few pages. Leary is interested almost solely in the more complex integrated processes and reactions.

A reading of the title headings would give one a very inadequate idea of the contents of the volume. Part 1 deals with Fundamental Structures, Drives and Mechanisms, the last two topics coming in for the major part of the discussion. Part 2 is entitled Intelligence: Its Nature and Functioning. In this section Leary discusses the problems of learning, language, "the type of behavior called mental," from both the normal and the abnormal aspects. The title of The Personality and Its Disturbances stands for Part 3. In this section, the treatment is largely from the psychoanalytic point of view, which the author insists has been widely misunderstood by many of its adherents as well as by most of its opponents, because this point of view has seldom been divorced from the emotional reactions of those who discussed it. The last part, under the heading Types and Systems of Personality Adjustment, contains statements of the adjustments of the personality to such matters as beliefs, religion, art and philosophy.

The reviewer believes that Leary has fallen into the all-too-frequent error of the behaviorists of failing to understand just what is meant by systematic experimental introspection. Thus his discussion and criticism of introspection on pages 133 ff. and elsewhere loses much of its point. The sort of verbal report to the question of "What time did you get up this morning?" can only be considered *Kundgabe* and not introspection at all. Furthermore, the statement, "Here, you will find yourself much less glib, unless you are accustomed to just uncritically rolling off a number of words representative in a very dubious sense of this past experience" (p. 134) is just as unfair in its context as it is out of it.

The book contains a number of new treatments of problems such as learning, personality and the social aspects, written, from new points of view. There are a great many footnote references throughout and a bibliography for further reading at the end of each chapter.

University of Pennsylvania SAMUEL W. FERNBERGER

The Psychology of Individual Differences. By R. S. ELLIS. New York, D. Appleton & Co., 1928. Pp. xxiii, 533.

An attempt is made to study individual differences over a wide range of topics. The book is elementary in character, and is more in the line of an extensive summary than a contribution of original ideas. It is written for the beginner or layman rather than the advanced student.

Topics dealt with include mainly the measurements of and differences among mental traits, simple and complex; environment *vs.* heredity; race, sex, and age differences; genius, deficiency, and insanity; brain structure and neurology, glands, and the biology and mechanics of inheritance; and applications of findings on differences to education, law, government, and business.

In general the author applies to nearly every conceivable function the normal distribution curve. Qualitative differences are not admitted except in rare cases, such as physical sex differences, color-blindness, and other sense impairment. All differences are deviations from the normal, in a quantitative but that nature is itself merely a "first habit." One is almost surprised at the end of the book to have the writer speak of a "drawn battle;" yet it must be admitted that the considerations brought forth in this book are no more conclusive concerning the power of environment than are data concerning the significance of heredity, and that there is as much difference between interpretation and proof in the one case as in the other.

A secondary theme is that the Binet intelligence tests involve a very considerable appeal to the capacity to manipulate words, and that they test simply one type of intelligence. Mechanical and social intelligence are mentioned as two other types of intelligence. Besides depending largely upon verbal capacities, a child's intelligence score may be seriously lowered in consequence of some specific disability. Correct treatment of the latter is shown in a number of examples to be followed by marked improvement in the general intelligence scores.

Considerable optimism is expressed concerning the possible effects of improved means and methods in educational procedure. After an excellent analysis of the relationship of intelligence to delinquency, the author concludes that much juvenile delinquency could be avoided by introducing the curricula employed in some reformatory schools into the fourth to sixth grades of city schools, for the benefit particularly of children of I.Q. 85–100.

University of Illinois HERBERT WOODROW

Sex and Repression in Savage Society. By BRONISLAW MALINOWSKI. New York, Harcourt, Brace & Co., 1927. Pp. xii, 285.

In recent years Malinowski has published a series of books on different aspects of primitive society, based upon original researches among the island communities of North-West Melanesia. The study of sex and repression is particularly deserving the attention of the anthropologist and the psychologist alike.

Malinowski's study of the family life of the Trobriand Islanders offers a valuable antidote to the rather uncritical and injudicious application of psychoanalytical theories to the explanation of cultural phenomena, as made by such writers as Freud, Roheim, Jones and others. By comparing the family organization of Melanesian society with that of our civilisation Malinowski shows clearly that the so-called "family complex" varies with the constitution of the family and the social stratum. Thus the Oedipus complex "corresponds essentially to a patrilineal Aryan family with the developed *patria potestas*, buttressed by Roman law and Christian morals;" whereas among the Melanesians with their matrilineal family organization and peculiar sex taboos and freedoms an essentially different complex prevails. (Called by him the matrilineal complex.) Malinowski makes a real contribution in pointing out that the typical configurations of sentiments and the problems of repression which the analytical school discovered can not be proclaimed a fundamental attribute of the human psyche. Different conditions and social situations will invariably and, as he shows, by necessity lead to different sentiments and complexes. The consequences of this conclusion are far-reaching since they apply to any attempt at psychological interpretation of human behavior.

To the cautioning notes previously sounded by Rivers, Dewey and Max Weber, there is added by Malinowski additional confirmation of the important methodological proposition that thorough knowledge of the cultural background of a group is necessary before a psychological interpretation can be attempted.

A further contribution is made by Malinowski in the last two parts of the book, in which he shows the inadequacy and fantastic nature of Freud's hypotheses as to the origin of certain cultural phenomena, and complexes in particular. Malinowski instead proposes a much more plausible thesis, built upon the assumption that the beginning of culture implies the repression of instincts by showing that all "complexes" are necessary by-products in the process of the gradual formation of culture.

University of Illinois THEODORE ABEL

Psychoanalysis and Education. By BARBARA LOW. New York, Harcourt, Brace & Co., 1928. Pp. 224.

This book possesses all the vigor and confidence (and perhaps the bias) of a stout defender of the faith. It is an exposition of a normal Freudian view of the dynamics as well as the organization of conscious and unconscious life. Its mission is to present the contributions of Freud to the direct and immediate purposes of education. To this end it explains to the teacher the nature and significance of the Freudian view of unconscious mind, the effect of unconscious wishes on behavior and personality patterns, the interrelations of emotion and intellect, and the mechanism of sublimation. It also presents the case for a large increase in actual use of psychoanalysis as an educational technique. The author points out that a teacher who does not understand himself stands little chance of understanding his pupils, hence true insight into one's self is a first step in a teacher's professional training. If our educational enterprises are to affect the whole child they must not be directed at advancing mere book-learning, nor must they deal with overt behavior and skills in a naïve and superficial way. Unless a child is to drift through school with his very 'self' unknown and hence unaffected except in chance ways, which are often sheer inadvertencies, his teacher must know and deal consciously with the hidden sources of his being. To do this his teacher must know enough about the Freudian view to recognize what is going on within the child and he should refer him promptly to the expert analyst when need for such reference is apparent. Further, certain shifts in emphasis in the whole range of present educational techniques will follow an appreciation of the Freudian interpretation of human life.

The book is well written; it has a message; it makes many a lecture in an average teacher-training psychology course look stale and sour. It deals with a genuine issue and points the way to a whole new set of theorems upon which to base educational practice—if the Freudian interpretation is the way to truth. The reviewer is somewhat impressed with the "if," however.

State University of Iowa F. B. KNIGHT

subject of the book, is forgotten in the brevity of the rule, which, when the book's obvious purpose is taken into consideration, may be good logic but dubious psychological method.

The chapters are organized after the conventional pattern: sensation, ideational types, reproduction, narration, lies, association and perception are treated before developing the contributions of the Teutonic school to phantasy and the "individual."

The chapter on intelligence and its testing postulates that there is not one intelligence, but rather various intelligences, yet still claims the three mental types: the creative, the concrete, and the composite imagery types. One supposes that Bobertag's revision of the Binet-Simon Scale is the only test that the central European countries know about. At least, it is the only one discussed, and its advised use is limited to selecting pupils for free places in schools. This omission may be an expression of disinterest in, or perhaps, disbelief in, the technique of mental and educational measurements.

As a textbook, this potpourri of axiomatic renderings of G. Stanley Hall's unproved if brilliant guesses, of early Wundtian psychology, and of the psychology of the "individual," is hardly adequate to any needs such as are felt in classes in American teachers colleges and in departments of education in universities.

Institute of Educational Research
Columbia University IRVING LORGE

Psychologie der frühen Kindheit bis zum sechsten Lebensjahre. By WILLIAM STERN. Fifth edition. Leipzig, Quelle & Meyer, 1928. Pp. xv, 539.

In attempting to present in this edition the various contributions of recent investigations in child psychology, the author has necessarily made the book diverge somewhat from the original form of the work. One of the chief contributions of the present edition is the greater elaboration and clarification of the author's view, that the child must be considered as an evolving personality, rather than as a mere composite of separate functions and traits. From this organic-unity or personalistic point of view, which he has stressed of late years, the author considers the recent contributions by investigators of thinking and intelligence and of the development of the child's esthetic life and enlarging ideational system. He also evaluates critically from this point of view the contributions of *Gestalt* psychology, of differential psychology, and of psychoanalysis. Several chapters have been largely rewritten in the light of the rapidly growing modern literature on experimental and observational studies of children's behavior. It is, of course, hardly to be expected that the numerous recent contributions by American investigators in this and in related lines will be largely and thoroughly incorporated in such a revised volume. Several important studies, however, come into consideration here and there. The book gives a rather inclusive summary of the behavior-traits and modifications of the growing child as revealed from a broad genetic point of view. The bibliography of this edition covers over ten pages, mostly of references to European literature.

J. P.

Clinical and Abnormal Psychology. By J. E. WALLACE WALLIN. New York, Houghton Mifflin Co., 1927. Pp. xiv, 649.

This book should put a wholesomely dampening influence upon the unhealthy curiosity that a great many students bring to the study of the abnormal. Clinical and abnormal psychology is treated as a dignified branch of science which deserves the attention of mature students of education and psychology. The book may serve equally well as a text-book or as a reference book. To one who has been exposed too early to the belittling influences of psychiatric promoters toward clinical and abnormal psychologists, this book should serve as a strong stimulant toward helping him see the value and the application of his chosen field. Although the book appears relatively small it is in reality large. The method of presentation of the material has aided appreciably in increasing the amount of ground covered. Wallin has produced a fine mixture of clinical and abnormal psychology which indicates clearly how we should think about the practice of psychometry. The clinical psychologist should have a background in education and sociology as well as training in all branches of psychology and psychiatry. The usual criticism may be offered, namely, that certain aspects of the field are inadequately handled. Clinical psychology has grown to include a study of all kinds of unusual behavior, such as occur in cases of special disability, emotional maladjustment, speech defect, physical handicap, etc., so that any one particular specialist might feel that his field is slighted in this book. But when one part is considered in connection with every other part of the book it will be apparent that there is no great error in the balance and proportion of parts.

A bibliography, limited to the more important recent text-books, treatises, and monographs appears at the end of the book.

University of Iowa LEE EDWARD TRAVIS

Practical Psychology: For Students of Education. By CHARLES FOX. New York, Harcourt, Brace & Co., 1928. Pp. xvii, 180.

The title of this book is misleading. One would never suspect from the name alone that it is intended to be a laboratory manual in educational psychology. The only excuse for its title is in the fact that the author aimed to make the subject-matter "as concrete and as little artificial as possible, consistent with scientific treatment."

The experiments, 28 in number, are of a semi-popular nature. They require little or no apparatus outside the book itself, and they are so intensely interesting that the reviewer could not refrain from doing some of them during the first reading of the volume. Indeed, they might easily furnish the basis for an evening's social entertainment. Yet, the students will not fail to get the point of each experiment in the laboratory, and the methods of treating results leave nothing to be desired, even by the most critical psychologist.

The subjects covered, and the number of experiments devoted to each one, are as follows: sensory acuity, 2; perception, 2; habit-formation, 5; imagery, 5; association and memory, 4; suggestion, 3; mental tests, 4; practice and fatigue effects, 3.

Most of the experiments are new, and yet they have been used long enough in the Cambridge laboratory to establish their value and to set up norms wherever norms are desirable. The author insists upon rigid quantitative treatment of results everywhere possible. To assist the student in this, Part II of the manual is devoted to statistical methods, including measures of relationship, averages and deviations. The very brief and yet advanced treatment of these subjects stands in sharp contrast to the elementary nature of the experiments themselves.

The usefulness of the manual would undoubtedly be enhanced by a correlated use of the same author's *Educational Psychology.* While it may not be adaptable *in toto* to American soil, certainly it will be very suggestive to anyone looking for new ideas in laboratory procedure in educational psychology.

University of Nebraska J. P. GUILFORD

Handbuch der Individualpsychologie. Edited by ERWIN WEXBERG. München, J. F. Bergmann, 1926. Vol. I, pp. ix, 664; Vol. II, pp. 200.

In two volumes thirty-one different authors contribute here to a handbook of individual psychology. The basic conceptions of Alfred Adler are used here not only in the fields of education, child psychology, and psychopathology, but also in considering such fields as ethics, religion, and philosophy. The reader will be hardly convinced that a reorientation of these various fields from the point of view of individual psychology has been achieved; the book rather shows what happens if theories formulated in connection with the development of therapeutically effective measures are suddenly introduced into such heterogeneous fields as politics, sexology, criminology, and philosophy, etc. The reader learns how the authors impressed by the importance of the *Leitlinie*, the *Gemeinschaftsgefühl*, etc., "feel" about the various kinds of scientific and non-scientific activity. The book does not show what is *gegeben* scientifically from a new point of view, the point of view of individual psychology, but demonstrates what is *aufgegeben* educationally, therapeutically, socially, and politically. Whoever has to handle human material will find a wealth of observations and suggestions in this book. The "bibliography of individual psychology" containing 337 references will be much appreciated.

Institute for Juvenile Research,
Chicago, Illinois HEINRICH KLÜVER

The Psychology of Learning: An Advanced Text in Educational Psychology. By. W. H. PYLE. Revised and enlarged. Baltimore, Warwick & York, 1928, Pp. ix, 441.

The revisions are slight. The original point of view and general outline remain. Two chapters are added, one on the Psychology of Special Subjects, consisting of five pages of statements which are so general as to be valueless; the other, on A Theory of Learning, is only a reprint of the article by the author published in *Psychological Review*, 1924. Neither chapter contributes any data whatever, or any suggestions as to scientific methods. The main additions in this revision are an enumeration of certain "general principles" and illustrative cases of their application, at the conclusion of each chapter. Many of the alleged principles and examples are put in the form of dicta: "The teacher should," etc., and "The child should," so and so. These are merely arbitrary statements. If much time is to be spent on learning these "principles," which is a danger when the class is in poor hands, the book will surely do little to advance scientific work in psychology. It may do much harm by encouraging dogmatic attitudes. There is no obvious reason why data for educational students, and scientific methods, should not be as well established as those for other students. Bibliography and references at the end of chapters are brought up to date, but are evidently not intended to be complete.

J. P.

Children's Behavior and Teachers' Attitudes. By E. K. WICKMAN. New York, Commonwealth Fund, Division of Publications, 1928. Pp. 247.

This is an exceptionally able and well organized inquiry into the nature and frequency of misbehaviors in ordinary school life together with readable presentations of the basic data and prudent interpretation of the data. That teachers should revise their appraisals of the relative seriousness of several 'offenses' or that our clinical experts are seriously incompetent to judge the true significance of many types of misbehavior is evident. The discussions on the analysis of the misbehaviors and teachers' attitudes toward them seem to the reviewer both forceful and correct. This document furnishes a much needed array of data for use in counseling teachers on the problems of misbehavior and its causes. We may yet persuade teacher-training institutions to concern themselves with the dynamics of human nature more and the dry bones of formalisms less, and this book will contribute valiant service to that end. The statistical treatment is on the whole sophisticated and a refreshing relief from the naïve incompetence of much work in this field. In the opinion of the reviewer the Commonwealth Fund got more for its money in this study than it often does.

State University of Iowa F. B. KNIGHT

Laura Bridgman: The story of an opened door. By LAURA E. RICHARDS. New York, D. Appleton & Co., 1928. Pp. xx, 155.

Now that the original accounts are out of print, psychologists and educators will welcome this well-written story of the development of one who was blind, deaf, dumb and virtually wanting in taste or smell. Mrs. Richards does not attempt an interpretation of the incidents she recounts, but her material suggestes the functioning of highly dynamic forces, often intensified by Laura's lack of normal human intercourse. One can plainly see energetic self-assertion with typical revolt at restraint, and compensation through efficient execution of activities within her powers; high satisfaction in established routine and severe attacks of homesickness when her environment was changed; craving for affection and some incipient romances; fairly clear stages in the evolution of a conscience and an intense interest in religious questions whose natural development might have contributed to our knowledge had not an over-zealous teacher imbued her with Calvinism.

Mt. Holyoke College S. P. HAYES

The Technique of Controversy: Principles of Dynamic Logic. By Boris B. Bogoslovsky. New York, Harcourt, Brace & Co., 1928. Pp. viii, 266.

This purports to be a "grammar of thought." The author derives from conspicuously successful examples of modern reasoning—Maxwell's electromagnetic theory of light, the concepts of species in biology and elements in chemistry, Dewey's interest and effort—a dynamic logic in contrast to the classical treatment of the subject which he labels static logic. The ancient sophism and the modern paradox are illogical only in that they contradict this static logic. Actually they conform to a world of ceaseless but interrelated change. Bogoslovsky revises the rules of reasoning, in general, to conform to such a world.

The chapter on principles of dynamic logic gives a good discussion of the psychology of reasoning. Reasoning as a process is generalized into an ingenious formula utilizing mathematical notation. The application of the principles to present day problems is characterized by apt illustrations and keen psychological insight. No small amount of controversy, particularly in the various fields of social science is traced to the attempt to treat relativistic and changing phenomena according to static canons of logic. The principles of dynamic logic provide not only an explanation of the origin of many controversies but a means of resolving some of them. Probably a new "grammar of thought" will no more insure accurate reasoning than other grammars insure correct use of language, but so far as a new grammar is better founded than the old ones it becomes a more useful criterion against which to evaluate differences of opinion.

University of Oregon — Howard R. Taylor

Notes on The Originality of Thought: The Concept of Internal Necessity: Poetic Thought and Constructive Thought. By Leone Vivante. Translated by Professor Brodrick-Bullock. London, John Lane, 1927. Pp. 221.

This work, which was reviewed by the undersigned when it appeared, in its original Italian form, is now made available in English. Although the reviewer is not competent to judge the adequacy of translation in every small detail, he is impressed by the facility of expression and the clarity of the presentation in spite of the abstruseness of the subject. As a psychological inquiry the book has relatively little value from an empirical point of view. The theoretical analysis of poetic thought follows the Bergsonian interpretation but from a new angle.

University of Iowa — Christian A. Ruckmick

Social Work and the Training of Social Workers. By Sydnor H. Walker. Chapel Hill, N. C., Univ. of North Carolina Press, 1928. Pp. xiii, 241.

The present status of the social worker is well delineated in this study which should prove of practical value to those who contemplate entering the field of social work. The book is illuminating to those who seek general information as to its problems and philosophy. A bibliography of thirty-five pages is appended.

University of Illinois — Herbert Woodrow

Sexualtyp und Kultur. By ERNEST KLIMOWSKY. Berlin, A. Marcus and E. Weber, 1928. Pp. 63. (Abhandlungen aus dem Gebiete der Sexualforschung, Band V, Heft 3.)

Within these sixty-three pages the author has attempted to present the sexually determined characteristics of men and women as they are revealed by music, literature, and the other arts through all the years from about the eleventh century to the present. In its brevity there lies the virtue of leading the reader to envisage easily the changing traits of the male and the female down through the centuries. At one time masculine traits predominated, at another feminine and again the two are equally balanced. But a larger study of the same subject might have been more convincing. There is no systematic survey to determine just what is masculine and what is feminine. That is taken rather for granted and traditional distinctions are used. The many references and quotations indicate a commendable erudition, but the psychologically trained reader will marvel at the omission of nearly all of the well known literature on the psychology of sex.

University of Oregon EDMUND S. CONKLIN

Directory of Psychiatric Clinics for Children in the United States. ANON. Second edition. Foreword by GRAHAM R. TAYLOR. New York, The Commonwealth Fund, Division of Publications, 1928. Pp. 181.

This new edition, which is the work of the Division on Community Clinics of the National Committee for Mental Hygiene, will be welcome by psychologists, psychiatrists, educators, etc., because the first edition (1925) has been out of print for some time. The first edition was published by the Joint Committee on Methods of Preventing Delinquency, in coöperation with the National Committee for Mental Hygiene; but with the termination of the demonstration period of the Commonwealth program, the first named of these two committees was discontinued and its activities were transferred to the newly created Division of Publications of the Commonwealth Fund. The Directory contains information gathered during the three months ending June 1, 1928, as well as certain additional information which became known even later to those persons handling the revisions, and it is therefore fairly representative of actual conditions at present and will be serviceable for several purposes.

J. P.

The Free Association Test (Kent-Rosanoff). By AARON J. ROSANOFF. New York, John Wiley & Sons, Inc., 1927. Pp. 546-620.

This is a reprint, separately bound, of Chapter VII of the sixth edition of the *Manual of Psychiatry*, edited by the author. The sixth edition has already been reviewed in the *Journal* by Edmund S. Conklin (cf. p. 130 above). The present chapter contains the same material as Appendix VI of the fifth edition (1920), and also four new pages of Juvenile Reactions. These new lists include all the reactions given in Woodrow and Lowell's *Children's Association Frequency Tables* (1916) that have a frequency value of over 0.1%. Doubtful reactions have not been included here.

J. P.

NOTES AND DISCUSSIONS

'OBSERVER' AND 'SUBJECT'

A recent proposal, made by an exponent of "consistent objectivism," to remove from the psychological laboratory the term *observer* and to retain only *subject* has suggested the following comment upon the history and the significance of these two words. Since the terms have long been carefully distinguished in many laboratories and by certain of our psychological periodicals, this comment will only formulate and discuss that which is already widely recognized in current practice.

Where the chief results of experiment have appeared in the form of verbal report, made under instruction, it is natural that the historical word *Versuchsperson* should have been commonly rendered 'observer.' There the psychological matter-of-fact is observed under a tacit agreement between *the experimenter*, who carries the general responsibility for the arrangements and the procedure and gives signals and instructions, and *the observer*, who works with whatever knowledge and sophistication the character and the aim of the problem dictate. The observer adjusts himself to the experimental situation, observes responsibly and with intent, and reports in prescribed terms and under prescribed rubrics. Although the observer's reports have frequently been made in terms of 'consciousness' and in the form of 'introspection,' these terms have absolutely nothing to do with the main functions of the psychological observer, who has precisely the same experimental significance and validity whatever the terms and the categories of report. Herein lies the persistent error of the 'objectivist' who naïvely insists upon identifying all reports of observers with 'subjectivism' and 'introspectionism.' Can it be possible that he has never learned at first-hand the actual experimental procedure of reporting in the psychological laboratory? It is difficult otherwise to understand his confusion. Some one must report in the psychological experiment. That is as obvious as it is in the physical, the chemical, and the physiological experiment. The point of the objectivist seems to be that he prefers to do all the reporting and recording himself and not to trust another. What he demands in his laboratory is the *Versuchstier*; some creature which he can 'put through' or 'run,' as he colloquially phrases it. Since that sort of behaviorism came out of the animal laboratory and from the Cattellian anthropometric test, it is natural that the paradigm of procedure should be the rat's maze and the freshman waiting line. The creature so 'run' pantomimes his part, cautioned not to say anything lest he introspect or prattle of conscious vacuities. At most he is permitted to verbalize with his laryngeal muscles or mutely to gesticulate in order to 'express' what is swelling within him. This creature the objectivist prefers to call *the subject*, overlooking the mild inconsistency between having subjects and rejecting with phobiac scorn everything 'subjective.'

In those experiments where *the observer* observes and reports, the setting is different. There a part at least of the results come out in the reports. The observer need know nothing of consciousness or of the various historical forms of introspection; but he must apprehend the experimental situation, he must comprehend the rules of procedure, he must take the experimental attitude, he must intelligently understand formal instructions set him by the experimenter, he must recognize the influence upon the experimental outcome of his surroundings (occasional instruction) and must detect the various and insidious forms of self-instruction which modify his reports, influence the amount of his accomplishment, and determine the character of his behavior. Finally, the observer must possess the linguistic resources necessary to accurate and appropriate recital. In a word, the psychological observer is always specially and technically trained; but his training is in principle the training of the reliable observer in any of the descriptive sciences. Some laboratories do doubtless speak still in the phraseology of mind, consciousness and introspection; though their number is obviously decreasing. If they make good psychological use of these concepts, there would seem to be no sufficient reason why the ardent objectivist should question their methodology. The point in the present context is that the employment and the training of the psychological observer have nothing in the world to do with these or with any other 'subjective' terms.

Now it is obvious, both from the logic of experimentation and from the actual output of our laboratories, that there are certain problems which are properly approached by the 'running of subjects,' human and animal. These are generally problems which may be phrased in the stimulus-response relation. They have been in all our laboratories for a half-century. All responsible experimentalists are able to deal with them. They require no special doctrine and no allegiance to a cult of objectivism. Their point of view was clearly stated by Herbert Spencer in 1855. The experimenter's interest is fixed upon environmental change, and the organism is regarded as a more or less constant third term set between the stimulating agent and the resulting performance or behavior. Here the organism may properly be regarded as 'subject.' But it is again just as obvious that the canons of experimental control and record make such an assumption inadequate and unprofitable in many problems of a different character. This is true wherever the organism enters the scene as *agent*, whether experiencing agent or agent of some other sort. There it is important that the history, the momentary disposition and attitude, the forward-running tendency and intent, and the general resources of the organism should be experimentally accounted for; and there it is usually most profitable to regard the organism as an observer and to make its functions of observation and report complementary to those of the experimenter. In many cases no other means have been provided for discovering many important factors which are involved in the experimental situation.

It is not to be expected that psychologists of all schools and of all parties will succeed in using in a competent way both 'subjects' and 'observers;' but it should be possible for all experimentalists at least to understand the distinction. Possibly we may hope that when the distinction is more clearly apprehended by the 'objectivist,' he will be willing to modify the old and badly conceived opposition between himself and an imaginary body of 'subjectivists.'

MADISON BENTLEY

(3) The large program will make the Proceedings a cross-section and a census of the psychology of the present day, certainly for America, and somewhat inaccurately for the world at large. The topics for the sessions were only in a small measure determined by the Program Committee; largely they were the result of a classification of the papers received and thus a census of the range of interests of the membership. What was psychology in 1929? The answer to this question will be the exhibit of the record of this program.

(4) One other generalization the writer ventures. The program seemed to him to show the increasing dominance of physiological, behavioral psychology in America. The day of behaviorism in America is over. It has accomplished its purpose, and American psychology now takes of physiological or behavioral data whatever its problems need. The same tendency seems to be not nearly so marked abroad, so that American psychology is now apt to have a flavor different from most of the European psychology.

The next Congress will be held in Copenhagen at the end of August, 1932. The Ninth Congress expressed its hope that Professor Höffding could be president of the Tenth.

E. G. B.

Illusory Movement from a Rotating Barber Pole

If one observes a rotating barber pole, preferably one of the type which is enclosed in a glass tube, three possible phenomena may occur. (1) The observer may see the movement as it objectively occurs, that is a torsion which is conditioned by the turning of the pole on its vertical axis. The diagonal stripes are taken for what they actually are, namely, spiral stripes on a revolving cylinder.

(2) The observer may see movement in the *vertical* direction only—an illusion which is conditioned by the spiral stripes as they appear to rise upon the turning pole. In this case, horizontal movement is missing. The pole seems to be sliding smoothly upward, and there is for the writer an imaginal extension of the pole above and below the glass case. The total perception is of a non-rotating pole of indeterminate length which is being shoved upward through the glass case. The extension of the pole is one of meaning rather than of visual quality. It is as if the portions of the pole above and below the glass case went out of view behind something, but there is the impression of movement behind that something.

(3) The observer may see a combination of the horizontal and vertical movements, with one of the two components prevailing to a greater or lesser degree. If the vertical component dominates, the stripes themselves seem to move upward on the surface of the pole, but the pole itself does not move upward as in (2). If the horizontal movement is dominant, the stripes seem to move diagonally upward in a direction at right angles to the stripes themselves.

The three phenomena may fluctuate, one giving way to another, as in the case of all ambiguous illusions. The fluctuations are also subject to the control of mental set, just as the fluctuations of the ordinary ambiguous figures, like the Necker-Wheatstone cube or the Schroeder staircase.

The writer has no objective proof of the rôle of eye-movements in these phenomena, but he believes that they have a great deal to do with the prevailing pattern of movement, or are at least a sufficient condition for them. Experimental registration of eye-movements is needed to decide the matter.

University of Nebraska J. P. GUILFORD

The Rate of the Hiccough Reflex

Last spring I suffered an attack of hiccoughs which persisted for over forty hours. The attack began at 8:30 A.M. on April 21, and ended during sleep sometime after 1:00 A.M. on April 23. During the first day I directed my efforts toward obtaining a cure, but when it became evident that none of the numerous 'cures' that I tried would be immediately effective I decided—particularly as I could do no concerted work under the distraction of the hiccoughs—to make the best of a bad situation and to study the temporal sequence of the reflex.

I timed the intervals between successive 'hics' during the 25th, 36th, and 40th hours of the attack. The results appear below.

Table Showing the Time Intervals between Successive Reflexes, the Number of Cases Occurring at Every Interval during the Three Periods Timed, the Average Time and the M.V.

Timed during the	Interval in seconds														Av.	m.v.
	Short			Medium					Long				Total			
	2	3	4	5	6	7	8	9	10	11	12	13–19	No.	Time*		
25th hr.	0	1	15	27	35	75	35	25	10	5	1	0	229	25′45″	6.75″	1.55″
36th hr.	1	20	48	61	36	33	17	12	6	1	0	0	235	22′	5.62″	1.46″
40th hr.	0	2	11	27	37	60	52	22	14	5	3	8	241	29′57″	7.46″	1.62″
TOTAL	1	23	74	115	108	168	104	59	30	11	4	8	705	77′42″	6.70″	1.59″

*I planned to time the intervals during every period for half an hour, but was interrupted during the first two periods.

As the table shows, the average time of the intervals between successive reflexes varied slightly for the three different periods. During the 25th hour of the attack the reflexes occurred on the average at the rate of one every 6.75 sec.; during the 36th hour they occurred every 5.62 sec.; and during the 40th hour every 7.46 sec. Although my diaphram was sore from the prolonged spasms, the soreness did not greatly affect the rate of the reflex. The rate was, to be sure, slowest during the 40th hour period, but the difference between the 40th hour and the other periods is not great, and moreover the rate was more rapid during the 36th hour than during the 25th. The variations from the averages in all the periods is, as shown by the m.v. approximately the same. The variations are large, being in all cases about 25% of the averages.

The data of the first two periods fit, as the table also shows, nicely into a normal curve of distribution, but that of the third period—which proved to be near the end of the attack—is skewed slightly to the right and tails off in the long intervals.

An inspection of the data shows that the rate varied gradually. Short intervals tended to follow short intervals and long to follow long. In order to test this result I divided the intervals, as shown in the table, into three classes: short, medium, and long. I then averaged for each of the 'short' and the 'long' classes (1) the times of the first following intervals; (2) the times of the second following intervals; and (3) the times of the first and second following intervals. The average of the first intervals following a short interval was 5.93 ± 1.25 sec.; of the second intervals, 5.67 ± 1.40 sec.; and of the first and second intervals, 5.80 ± 1.34 sec. The average of the first intervals following a long interval was 7.79 ± 1.51 sec.; of the second intervals, 7.76 ± 1.61; and of the first and second intervals, 7.78 ± 1.56. The average of the intervals following long intervals, whether the first, second, or first and second are considered, is approximately 2 sec. longer than the intervals following short intervals.

The results of this study, which was inopportunely interrupted, show that the temporal interval between successive hiccough reflexes is, in my case, approximately 7 sec.; that there is a gradual and large variation in the rate; and that the rate is little subject to fatigue. I venture to report these results in the hope that someone will be prompted to undertake a more prolonged study. If the intervals were timed over a longer period—a week or ten days—much positive knowledge, I believe, would be gained regarding the rate and rhythm of this reflex, and the effects of fatigue upon it.

K. M. D.

SOCIAL SCIENCE RESEARCH COUNCIL'S GRANTS-IN-AID

The Social Science Research Council announces its second annual list of Grants-in-Aid. Altogether, thirty-seven people received support. On the basis of major emphasis, their projects were distributed among the social science as follows: Anthropology 2, Economics 6, History 16, Political Science 2, Psychology 2, Sociology 5, Human Geography 2. Most of the projects, needless to say, straddle a number of these fields. In fact, while not insisting upon it, those administering Grants-in-Aid look with favor upon projects that represent a broader approach to a problem than is possible within the limits of any one discipline.

The Grants-in-Aid of special interest to our readers are: Clark L. Hull (University of Wisconsin), "To work out experimentally some of the more important phenomena of normal waking suggestion;" Carney Landis (Wesleyan University), "An investigation of the underlying factors in individual social maladjustment, particularly the possibilities of using measurements of muscular and physiological reactions as determiners of, indicators of, or criteria of individual delinquency;" Fred A. Moss (George Washington Univeristy), "Statistical analysis of five thousand tests given in industry, and ten thousand tests given in educational institutions with the aim of determining the validity of the various items in the social intelligence test, with the ultimate aim of constructing a more valuable device for measuring ability to get along with others;" A. R. Gilliland and John J. B. Morgan (Northwestern University), "A test of introversion and extroversion as part of a program of developing measures of personality."

The aid granted by the Council ranges from $200 to $2000, and in almost every case is conditional upon liberal coöperation from the institution with which the applicant is connected.

The administration of Grants-in-Aid is in the hands of a special Committee of the Council known as the Committee on Grants-in-Aid. The Committee plans to hold two meetings during the coming academic year: in mid-November and in mid-March. The closing date for receiving applications for consideration at the latter meeting is February 1, 1930. Any person who is a resident of the United States or Canada and who has evidenced real research ability is eligible. The Committee expects that the applicant's project will be well under way, that he can forecast the time and the money required to complete it, that it is not to be used in fulfillment of any higher academic degree, and that he has already canvassed carefully the possibility of aid from his own institution. Applications should be addressed to Walter R. Sharp, Secretary of the Committee on Grants-in-Aid of the Social Science Research Council, 230 Park Avenue, New York City.

M. F. W.

DEUTSCHE GESELLSCHAFT FÜR PSYCHOLOGIE

In announcing its change of name from *Die Gesellschaft für experimentelle Psychologie* the above Society fervently pleads for a juster recognition of psychology by the German State and in institutions of higher learning. The announcement, which is signed by Karl Bühler (Vienna), William Stern (Hamburg), Narziss Ach (Göttingen), David Katz (Rostock), Johannes Lindworsky (Prague), Walter Poppelreuter (Bonn-Aachen), and Hans Volkelt (Leipzig), remarks upon recent changes of academic status in several German universities under which vacant chairs of psychology have been filled by pedagogues and philosophers. While these changes ultimately rest upon the sterility of an older and antiquated psychology, the Society contends that the subject is now vastly revised and extended and hence deserving of better academic treatment. The *apologia* notes the following modern improvements and extensions. Experimental research has been carried into the 'higher mental functions,' toward the artistic, moral and religious life, and to the subjects of character and temperament. Outside the realm of experiment have appeared the study of behavior and also social and genetic studies of man and of his products. Still maintaining its relations with the natural sciences, psychology has enriched itself by new and intimate relations with all the cultural sciences, with philosophy, phenomenology, epistemology and theory of value. Education, training, the care of dependents, and the administration of justice definitely imply the current psychology of childhood and youth. Vocation and the arts and instruments of work have been firmly placed upon a psychological foundation, while nearly every branch of medicine appropriately touches one or another phase of the same subject. German psychology, which has supplied the model and the inspiration for other lands, should not be embarrassed—as these men contend—but rather supported and cherished in its own home.

M. B.

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By J. P. GUILFORD, University of Nebraska

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